PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

From November 19, 1874, *to June* 17, 1875.

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ERRATA.

Page 398, line 18, *for* ·21560 *read* ·21506.

" head of column 3 of Table, *for* $12\frac{}{n}$ *read* $12\frac{r}{n}$.

Page 405, line 25, *for* //c=b=\\c♯ *read* //c=6[six]=\\c♯.

PROCEEDINGS

OF

THE ROYAL SOCIETY.

November 19, 1874.

WILLIAM SPOTTISWOODE, M.A., Treasurer and Vice-President, in the Chair.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair.

General Boileau, Mr. De La Rue, Capt. Evans, Dr. Gladstone, and the Right Hon. Lyon Playfair, having been nominated by the President, were elected by ballot Auditors of the Treasurer's Accounts on the part of the Society.

The decease of Mrs. Hooker having been mentioned from the Chair, Sir James Alderson proposed, and General Boileau seconded, the following Resolution, which was unanimously agreed to:—"That the Royal Society desire to condole with their President for his loss, and to express to him their deep sympathy in his great affliction."

Dr. Henry Wyldbore Rumsey was admitted into the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Note to the 'Report on the Exploration of Brixham Cave' (Phil. Trans. 1873)." By G. Busk, F.R.S., V.P.L.S. Received June 22, 1874.

With reference to Prof. Owen's correction of the erroneous English equivalent for *Lagomys spelœus* (which occurs, not in the Report on the Animal Remains, but in the "General Conclusions respecting the Brixham Cave," accompanied, however, with the proper scientific name), I have merely to remark that it is of course a very obvious erratum. It is one,

however, which from the context can lead to no mistake, and is consequently of no importance. Though the proof of that part of the "Report" passed under my eyes, the error entirely escaped my attention.

With regard, however, to Professor Owen's remark that throughout the "Report" the discovery of *Lagomys* in Brixham Cave is treated as an original discovery, I have simply to observe that the Report consists of one hundred pages, in which about five lines, at most, relate to *Lagomys spelœus*, in two of which the reporter observes, with perfect truth, that "it has been met with elsewhere in association with Pleistocene mammals." The statement of this well-known fact is hardly, in the usual sense of words, a claim to "original discovery."

II. "On the Tides of the Arctic Seas.—Part IV. The Tides of Northumberland Sound at the Northern Outlet of Wellington Channel.—Part V. The Tides of Refuge Cove in Wellington Channel." By the Rev. SAMUEL HAUGHTON, M.D. Dublin, D.C.L. Oxon. Received July 11, 1874.

(Abstract.)

These tidal observations were made by Sir Edward Belcher, R.N., K.C.B., on board of H.M.S. 'Assistance,' in the summer of 1853. They are interesting, from the fact that they were made in the highest latitudes in which tidal observations have been ever recorded.

From a discussion of the heights and times of high and low water the following partial results have been obtained, which, it is hoped, will be further extended and corrected, by taking into account the height of the water at other phases of the tides.

NORTHUMBERLAND SOUND.

I. *Diurnal Tide.*

1. True Solitidal Interval,

$$i_s = 7^h\ 49^m.$$

2. True Solar Coefficient, corrected for declination,

$$S = 4{\cdot}7 \text{ inches.}$$

II. *Semidiurnal Tide.*

1. Mean Lunitidal Interval (observed),

H. W.	L. W.
$+0^h\ 7^m{\cdot}05$	$6^h\ 35^m{\cdot}35.$

2. Difference between true Lunitidal and true Solitidal Intervals,

$$i_m - i_s = 38^m.$$

REFUGE COVE.

I. *Diurnal Tide.*

True Lunitidal Interval,

$$i_m = 20^h\ 48^m.$$

II. *Semidiurnal Tide.*

Mean Lunitidal Interval (observed),

H. W.	L. W.
$-0^{h}\ 26^{m}.7$.	$6^{h}\ 1^{m}.1$.

III. "On Musical Duodenes, or the Theory of Constructing Instruments with Fixed Tones in Just or Practically Just Intonation." By Alexander J. Ellis, F.R.S., F.S.A., F.C.P.S., F.C.P. Received October 28, 1874.

This paper is intended to complete and supplement three papers on Music which I have already read before the Royal Society[1]. It contains a more complete theory of temperament, embracing that indicated by Helmholtz[2], but not worked out by him, and its application to the theory of constructing musical instruments with an intonation practically just, without change of fingering, and, if there are three or four performers, without change of mechanism. The name *Duodene* refers to that collection of *twelve* notes, suitable to the present manuals, which is made the unit of construction. To obtain its precise form, and determine the number and value of all such duodenes as it is necessary to tune, I have been obliged to indicate a theory of harmonic scales and modulation, which I believe to be entirely new, and which has of course other uses. The great extent of the subject obliges me to confine this part of my paper to a mere indication.

A. *Notation of Pitch.*

The letters C, D, E, F, G, A, B indicate both musical tones and the number of vibrations made by the prime or lowest partial tone of each in a second; so that, C being known,

$$D=\tfrac{9}{8}\,C,\ E=\tfrac{5}{4}\,C,\ F=\tfrac{4}{3}\,C,\ G=\tfrac{3}{2}\,C,\ A=\tfrac{5}{3}\,C,\ B=\tfrac{15}{8}\,C.$$

The marks ♯ ♭ † ‡ ¶ ⁋ are used for fractional multipliers, having the following names and values:—

$$\text{sharp, ♯} = \frac{135}{128};\quad \text{flat, ♭} = \frac{128}{135},$$
$$\text{high, †} = \frac{81}{80};\quad \text{low, ‡} = \frac{80}{81},$$
$$\text{skhismic, ¶} = \frac{32805}{32768};\quad \text{hyposkhismic, ⁋} = \frac{32768}{32805},$$

[1] "On the Conditions, Extent, and Realization of a Perfect Musical Scale on Instruments with Fixed Tones," read Jan. 21, 1864, printed at length in Proceedings, vol. xiii. p. 93; "On the Physical Constitution and Relations of Musical Chords," and, lastly, "On the Temperament of Musical Instruments with Fixed Tones," both read on June 16, 1864, and printed at length in the Proceedings, vol. xiii. p. 392 and p. 404.

[2] Tonempfindungen, 3rd ed. p. 495.

the first two being written and read *after*, and the last four being written and read *before*, the letter which marks the note. Thus:—

$$\dagger A\flat = \text{high A flat} = \frac{81}{80}\cdot\frac{5}{3}\cdot\frac{128}{135}\cdot C = \frac{4}{5}C,$$

$$\ddagger G\sharp = \text{low G sharp} = \frac{80}{81}\cdot\frac{3}{2}\cdot\frac{135}{128}\cdot C = \frac{25}{16}C.$$

The precise pitch of every tone is therefore indicated by its symbol, when C is known.

When *Italic* letters are used, $C_{\prime}$, C, c, c', c'', &c. indicate Octaves, and C is the pitch of the lowest note of the violoncello. When Roman letters are used, no such relative values are attributed to large and small letters.

B. *Temperament.*

All intervals here considered can be made up of Fifths, major Thirds, and Octaves, taken up or down. In other words, the ratio of the vibration-numbers of any two tones can be represented by $\left(\frac{3}{2}\right)^m\cdot\left(\frac{5}{4}\right)^n\cdot 2^p$, where m, n, and p are zero, or some positive or negative integers. In Table I. the ratios of the vibrational numbers of all the tones in the same horizontal line is $\left(\frac{5}{4}\right)^n\cdot 2^p$, and in the same vertical column is $\left(\frac{3}{2}\right)^m\cdot 2^p$, the p or Octave being left indeterminate. If, then, we proceed from any note supposed to be the lower, first horizontally to the column containing a second note, supposed to be higher and in the same Octave, and then vertically to the second note itself, and multiply or divide by $\frac{5}{4}$ for each horizontal step, according as it is to the right or left, and multiply or divide by $\frac{3}{2}$ for each vertical step, according as it is upwards or downwards, and finally multiply or divide the result by 2, until the final result lies between 1 and 2, that fraction will be the ratio of the higher vibrational number to the lower. Thus C to F♯ gives C to E, $\frac{5}{4}$; E to B, $\frac{3}{2}$; B to F♯, $\frac{3}{2}$; and $\frac{5}{4}\cdot\left(\frac{3}{2}\right)^2 = \frac{45}{16}$, which, being greater than 2, on being divided by 2 gives the correct ratio of the *Tritone* C to F♯ as $\frac{45}{32}$. An inspection of the Table then shows that ‡D to ‡G♯, †G♭ to †C, &c. are the same intervals as C to F♯, because they have the same relative position, and this is sometimes very convenient.

But as 2, 3, 5 are primes, no one tone in Table I., supposing it to be indefinitely extended, will have the same pitch as any other tone. The object of Temperament generally is to obviate this inconvenience, by slightly altering the ratios of the Fifth or major Third, or both. From doing so, as Helmholtz has proved, more or less dissonance must result.

Another object, therefore, is to make that dissonance as little annoying as possible.

We find immediately by actual multiplication,

$$\left(\frac{3}{2}\right)^{12} \cdot \left(\frac{1}{2}\right)^{7} = \frac{531441}{524288} = †¶, \text{ the comma of Pythagoras,} \quad (1)$$

$$\left(\frac{4}{5}\right)^{3} \cdot 2 = \frac{128}{125} = ††|\!\flat, \text{ the diesis} \quad (2)$$

Multiplying these equations together, and extracting the cube root,

$$\left(\frac{3}{2}\right)^{4} \cdot \frac{4}{5} \cdot \left(\frac{1}{2}\right)^{2} = \frac{81}{80} = †, \text{ the comma (of Didymus)} \quad (3)$$

Dividing (1) by (3),

$$\left(\frac{3}{2}\right)^{8} \cdot \frac{5}{4} \cdot \left(\frac{1}{2}\right)^{5} = \frac{32805}{32768} = ¶, \text{ the skhisma.} \quad (4)$$

On these equations depend all *uniform* temperaments in which every Fifth and major Third preserves the same ratio throughout.

Let V, T, k, s be any four fractions having relations similar to $\frac{3}{2}$, $\frac{5}{4}$, † and ¶ respectively in (1) and (2); then

$$V^{12} \div 2^{7} = ks, \text{ and } 2 \div T^{3} = k^{2} \div s. \quad (5, 6)$$

Subtracting the logarithms of (1) and (2) from the logarithms of (5) and (6) respectively, we find

$$\log V = \log \frac{3}{2} - \frac{1}{12} \cdot (\log † - \log k + \log ¶ - \log s), \quad (7)$$

$$\log T = \log \frac{5}{4} + \frac{2}{3} \cdot (\log † - \log k) - \frac{1}{3} (\log ¶ - \log s), \quad (8)$$

which are the fundamental equations of all temperament, and are identities, of course, for just intonation. As they contain 4 unknown expressions, two may be assumed and the rest found, giving rise to an endless variety of temperaments. Without discussing these generally, the following cases should be mentioned :—

Commatic System (for which $k=1$).—This is the only system discussed in my previous paper, where 50 cases were considered.

1. *Quintal* or Pythagorean Temperament. Assume $k=1$, and $V=\frac{3}{2}$, then by (7),

$$\log † + \log ¶ = \log s;$$

whence by (8),

$$\log T = \log \frac{5}{4} + \log †.$$

This temperament proves to be thoroughly unsuitable for harmony.

2. *Tertian*, Mesotonic or Mean Temperament. Assume $k=1$ and $T=\frac{5}{4}$, then by (8),

$$2 \log † = \log ¶ - \log s;$$

whence by (7),

$$\log V = \log \tfrac{3}{2} - \tfrac{1}{4} \log \dagger.$$

In my previous paper I have shown this to be the best suited for harmony of all in this system; but it requires 27 tones to the Octave.

3. *Hemitonic* or Equal Temperament. Assume $k=1$, and also $s=1$, then by (7),

$$\log V = \log \tfrac{3}{2} - \tfrac{1}{12} \cdot (\log \dagger + \log \P),$$

by (8),

$$\log T = \log \tfrac{5}{4} + \tfrac{1}{3} \cdot (2 \log \dagger - \log \P).$$

Since $\log \dagger = 0{\cdot}005\ 3950\ 319$ (whence $\frac{1}{11} \log \dagger = 0{\cdot}000\ 4904\ 574$) and $\log \P = 0{\cdot}000\ 4901\ 071$, we may, for all acoustical purposes, assume $\log \P = \frac{1}{11} \log \dagger$, and hence that

$$\log V = \log \tfrac{3}{2} - \tfrac{1}{11} \cdot \log \dagger = \log \tfrac{3}{2} - \log \P,$$

$$\log T = \log \tfrac{5}{4} + \tfrac{7}{11} \cdot \log \dagger = \log \tfrac{5}{4} + 7 \log \P.$$

This is the only uniform temperament which requires no more than 12 tones, and hence, although very ill suited to harmony, it has been theoretically adopted by almost all musicians.

Skhismatic System (for which $s=1$).

4. *Skhismic* or Arabic, according to Helmholtz's indication (*op. cit.* p. 441). Assume $s=1$ and $V=\frac{3}{2}$; then by (7),

$$\log k = \log \dagger + \log \P,$$

and by (8),

$$\log T = \log \tfrac{5}{4} - \log \P.$$

The Arabs tune 17 tones to form 16 perfect Fifths, and thus obtain 8 major Thirds which are only one skhisma too flat. But they do not use harmony in our sense of the word, although this would give almost just intonation in certain keys.

5. *Skhistic*, or Helmholtzian, as it may be called, because suggested by Helmholtz (*op. cit.* p. 495). Assume $s=1$ and $T=\frac{5}{4}$; then by (8),

$$2 \log k = 2 \log \dagger - \log \P,$$

and by (7),

$$\log V = \log \tfrac{3}{2} - \tfrac{1}{8} \log \P.$$

The name *skhistic* is derived from *skhist*, or by abbreviation σ, which is

my name for $\sqrt[8]{\P}$. Since $\log \dagger = 88 \log \sigma$ very nearly, one comma may be said to contain 88 skhists. Skhistic temperament is indistinguishable from just intonation, and I shall use it in the theory of constructing instruments. If in Table I. we suppose the horizontal intervals to be still $\frac{5}{4}$, but the vertical intervals to be $\frac{3}{2\sigma}$, and the sign † to stand for $\frac{81}{80}\surd$♭, while $\dagger\ddagger=1$ as before, then this Table represents skhistic relations; so that if from any note, as E♭ (col. 5, line x), we proceed by 8 skhistic Fifths up to †B and then one major Third to the right, we find a tone D♯, which, when reduced to the same Octave, is identical with E♭. We thus find a number of *skhistic synonyms* shown in Tables II. & III.

We may express the *errors in the temperaments* just discussed in terms of skhists thus, using ♯56σ to mean "too sharp by 56 skhists," and so on, and 0 to mean "no error":—

Just.	1. Quintal.	2. Tertian.	3. Equal.	4. Skhismic.	5. Skhistic.
Minor Third	♭88σ	♭22σ	♭64σ	♯8σ	♭1σ
Major Third	♯88σ	0	♯56σ	♭8σ	0
Fourth	0	♯22σ	♯ 8σ	0	♯1σ
Fifth	0	♭22σ	♭ 8σ	0	♭1σ
Minor Sixth	♭88σ	0	♭56σ	♯8σ	0
Major Sixth	♯88σ	♯22σ	♯64σ	♭8σ	♯1σ

The error of one skhist is quite inappreciable by the most practised ears in melody, and can be detected harmonically only by very slow beats for tones in the highest Octaves used in music.

6. *Cyclic Temperaments.* The following method is far more general than that given in my previous paper (Proc. vol. xiii. p. 412). Put

$$m \,.\, \log \mathrm{V} = v \,.\, \log 2, \qquad m \,.\, \log k = q \,.\, \log 2,$$
$$m \,.\, \log \mathrm{T} = t \,.\, \log 2, \qquad m \,.\, \log s = z \,.\, \log 2,$$

and, after substituting these values for log V, log T, log k, log s in the logarithms of equations (5) and (6), divide out by log 2, and multiply up by m. Then

$$12v - 7m = q + z, \qquad m - 3t = 2q - z \quad \ldots\ldots\ldots \quad (9, 10)$$

Take any integral values for q and z, and find the integral values which satisfy one of these indeterminate equations for v, m, or t, m, and substitute in the other, taking the resulting integral values of t or v respectively. The five integral values determine a *cycle* in which the Octave is divided into m aliquot parts, which may be termed *octs*, v of which make a Fifth, t a major Third, q a comma, and z a skhisma of "the cycle of m." Most of the results are valueless, but the following present either theoretical convenience or historical interest:—

No.	Cycle of *m.*	Fifth, *v.*	Major Third, *t.*	Comma, *q.*	Skhisma, *z.*
1	30103	17609	9691	539	48
2	3010	1761	969	55	7
3	301	176	97	5	0
4	53	31	17	1	0
5	53	31	18	0	1
6	31	18	10	0	−1
7	12	7	4	0	0

Of these, the first three are here, I believe, for the first time shown to be true cyclic temperaments.

1. The cycle of 30103 is such an excellent representative of just intonation (giving even 6 octs for the skhist), that it can be used without sensible error, in place of ordinary logarithms, to reduce the relations of intervals to addition and subtraction, for general use among musicians or learners unacquainted with higher arithmetic. By dividing out by 100,000 we obtain almost precisely the five-figure logarithms of the intervals.

2. The cycle of 3010 is almost as correct, with smaller numbers.

3. The cycle of 301 is almost a perfect representation of skhistic temperament, in which the skhisma is eliminated, and for that reason becomes perhaps the most practical representation of general musical intonation.

4. The first cycle of 53 is Nicholas Mercator's representation of just intonation, but it is more correctly a representation of skhistic temperament, and not so good as No. 3[1].

5. The second cycle of 53 is a very accurate representation of Pythagorean intonation, and has actually been proposed for the violin by Drobisch.

6. The cycle of 31 is Huyghens's Cyclus Harmonicus, and closely represents the tertian or mean temperament.

7. The cycle of 12 is the ordinary equal temperament, and its principal convenience consists in the very small number of its octs, here called Semitones.

Unequal Temperaments, whether they consist of 12 selected tones from uniform temperaments, or of 12 tones turned intentionally false (see my former paper, Proc. vol. xiii. pp. 414–417, for their theory), are now abandoned. But the difficulty of tuning equal temperament by estimation of ear, or even by the monochord, and of retaining the intonation of the piano or organ unchanged for even an hour, makes all temperaments in actual use really unequal. The difficulty of original tuning by estimation

[1] When this paper was read, I mentioned that this was the cycle used by Mr. Bosanquet in his paper read before the Royal Society on 30th January, 1873.

of ear in the case of skhistic temperament, where the Fifths have to be flattened by an almost inaudible skhist, is so much enhanced as to be insuperable except by Scheibler's method[1]. Hence it is necessary to find a practical substitute. This I term

Unequally Just Intonation.—Suppose that the 48 tones marked off by a dotted line in Table I. have to be tuned in this substitute for skhistic intonation. Tune C to the fork. Take 4 just Fifths up (or Fourths down), C to G, G to D, D to †A, †A to †E, without beats; and three just Fifths down (or Fourths up), C to F, F to B♭, B♭ to E♭. Then tune C to E as a just major Third up, without beats, and from E proceed to its just Fifth B, verifying the result by determining that it is a just major Third above G, and so on to G♯ up and ‡G down. Then if from G♯ we proceeded to the just Fifth, D♯, the resulting tone would be exactly one skhisma sharper than E♭, whereas in skhistic intonation it would be identical with E♭, as already shown. It is needless to say that no tuner could effect this exact difference of a skhisma, but he will come practically near it, and the error is that of the Fifth (the least of the errors) in equal temperament. If we were to proceed in this way for all the six columns of 8 tones marked off in Table I., we should have just major Thirds throughout, and just Fifths also in all but 5 cases—namely, ‡D♯♯ to ‡B, B♯ to ‡G, G♯ to E♭, †E to C♭, and †C to †A♭♭, each of which would be too flat by one skhisma. Since ‡G (Table I. col. 6, line x) is a just major Third below ‡B, and a just minor Third above ‡E, but ‡E is a whole skhisma flatter than ‡D♯♯ (col. 8, line p), which would be played for ‡E, it follows that the minor Third, ‡D♯♯ to ‡G, would be a skhisma too flat or close; and similarly that the minor Thirds, B♯ to E♭, G♯ to C♭, and †E to †A♭♭, or 4 minor Thirds on the whole, would be a skhisma too flat[2]. Hence this style of tuning gives 5 Fifths and 4 minor Thirds, as

[1] Calculate the logarithms of the ratios of all the skhistic tones by perpetual addition or subtraction of 0·1760300 (=log $\frac{3}{2}$—log σ) to or from 0, continually adding or subtracting 0·3010300 (=log 2) to make the results positive and lie between this and 0. Add the logarithm of the vibrational number of C, and then find the numbers (to three places of decimals) corresponding to these logarithms. This gives the vibrational numbers of all the skhistic tones in the Octave, of which 48 will be required. Subtract 4 from each of these values, and procure tuning-forks giving exactly the tones thus determined to at least the hundredth of a vibration in a second. These may be obtained of the great manufacturer of acoustic apparatus, Mons. R. Koenig, of Paris; but it is necessary to state that the English and German (not the French) system of counting vibrations is to be used. Then tune each tone roughly to the corresponding fork, and afterwards *sharpen* it until it beats 4 times in a second with the fork. By this means, and by this means only, with great care and attention, the pitch may probably be obtained with sufficient accuracy to distinguish skhistic from just intonation. And similarly for equal and tertian intonation.

[2] Taking the cycle of 30103, ‡G contains 17070, ‡B 26761, and ‡D♯♯ 9200 octs. Hence the Fifth, ‡D♯♯ to ‡B, has only 17561 in place of 17609 octs, and the minor Third, ‡D♯♯ to ‡G, only 7870, in place of 7918 octs—that is, in each case 48 octs too little; that is, these intervals are one skhisma too flat.

bad as the *best* intervals (Fifths) in equal temperament, and all the other intervals absolutely just. Hence the name *unequally just.* In future I shall consider that skhistic intonation is practically realized by unequally just intonation, for which the practical rule is :—*tune six tones making just major Thirds without beats* (as †F♭ to †A♭ to C to E to ‡G♯ to ‡B♯, line *t* in Table I.), *and from each of them tune seven other tones making just Fifths* (as C to G to D to †A to †E, and C to F to B♭ to E♭, in col. 5 of Table I.).

Saunders's "Tilting Action."—Before proceeding to show that 48 skhistic tones suffice for modern modulational music, it will be useful for future constructions to remark that a method of realizing all the effects which I contemplated by my duplex finger-board (Proc. vol. xiii. p. 422) has been invented by Mr. T. W. Saunders[1], by means of stops, which allow the manual and fingering to remain unaltered. Two sets of harmonium vibrators are arranged one behind the other, tuned in tertian intonation (by means of beats, counted by a pendulum, which gives a fairly accurate means of approximating to the correct result) as follows, the capital letters referring to the white or long digitals, and the small letters to the black or short digitals, a mode of distinction which I shall constantly employ :—

Back	B♯	d♭	C♯♯	e♭	F♭	E♯	g♭	F♯♯	a♭	G♯♯	b♭	C♭
Front	C	c♯	D	d♯	E	F	f♯	G	g♯	A	a♯	B

There are 12 stops[2], one corresponding to each digital in the Octave, which, by a "tilting action," enables one, and one only, out of the two vibrators in the same column, as shown above, to be "damped" at pleasure throughout all the Octaves of the instrument. When all the stops are pushed in, the front vibrators only are free, and any one may be exchanged for a back vibrator by pulling out its stop. Hence 24 out of 27 tones are under the command of the player; B♭♭, E♭♭, A♭♭ are omitted.

C. *Harmonic Scales.*

A series of tones, each of which is consonant with two other tones in the same series that are themselves consonant with each other, forms what I here mean by an *harmonic scale.* This was not the principle on which scales were originally formed; but this is the way in which the pitch of the tones must be determined for the just intonation of modern harmony and modulation.

[1] As Mr. Saunders has not patented his invention, I am unable to give more than the indications in the text, and refer to him personally, at E. Lachenal's Concertina Manufactory, 4 Little James Street, Bedford Row, W.C. His invention offers great facilities for the construction of experimental instruments in any uniform or just intonation. His harmonium was shown when this paper was read.

[2] In the specimen shown there are only 9 stops, the C♯♯, F♯♯, G♯♯ having been omitted; but as the principle admits of the construction of 12 stops as easily as 9, the complete form is mentioned in the text.

The Harmonic Elements are the Fifth, C × G $\left(=\frac{3}{2}\right)$, the major Third, C + E $\left(=\frac{5}{4}\right)$, and the minor Third, C − †E♭ $\left(=\frac{6}{5}\right)$, using a notation which I have found practically very convenient for representing the intervals between two tones; the symbols ×, +, − are not to be employed with any other meaning between the names of tones. In these elements it is supposed that either note may be raised or depressed by any number of Octaves, or be accompanied by such Octaves of itself.

†E♭	G
	C	E

The Harmonic Cell, or Unit of Concord, consists of a major triad, C + E − G, and a minor triad, C − †E♭ + G, arranged as in the margin, and having the same First C, and hence the same Fifth G. The Fifth C × G is placed vertically, the two major thirds, C + E and †E♭ + G, are horizontal, and the two minor Thirds, C − †E♭ and E − G, slope obliquely from the bottom upwards to the left. These positions, then, replace the symbols ×, +, −. Allowing any one of the tones to be altered by any number of Octaves, or to have Octaves of itself added, and any tone to be taken as the First, this *cell,* whence all harmony is *developed,* contains every chord recognized by musicians as a concord in Tertian Harmony—that is, harmony depending on Octaves, Fifths, and *Thirds* alone, excluding natural Sevenths $\left(=\frac{7}{4}\right)$, which form Septimal Harmony. By Table I. cells can be readily constructed on any tone as a First.

†E♭	G	
†A♭	C	E
	F	A

The Harmonic Heptad, or Unit of Chord-relationship, consists of *two* cells, the First of one being the Fifth of the other, as in the margin. Allowing Octave variations as before, this contains all the three major and three minor triads which have C as one of their constituents, and are thus related in the first degree. Two of these chords, the minor triad A − C + E on the right or major side, and the major triad †A♭ + C − †E♭ on the left or minor side, connecting the two cells and due to their union, may be called *union triads,* to distinguish them from the four *cell triads.* The heptad also contains all *con-dissonant triads* (as I term them), consisting of three tones, two of which are consonant with C but dissonant with each other. Of these the *trine* †A♭ + C + E, which forms the central horizontal line, is most important for future work.

†B♭	D	
†E♭	G	B
†A♭	C	E
	F	A

The Harmonic Decad, or Unit of Harmony, consists of two heptads having a common cell, and hence of three cells, the Fifth of the first, lowest, or *subdominant* cell, and the Fifth of the second, middle, or *tonic* cell, being the First of the second cell, and First of the third, highest, or *dominant* cell, respectively. The decad contains three major and three minor cell triads, and two major and two minor union triads —that is, ten triads in all, together with all the discords possible without

modulation. The First of the tonic cell is called the *tonic of the decad*, and gives its name to it. The example, therefore, is a C decad.

Harmonic Trichordals consist of three triads, one from each cell in a decad, and form eight groups. Contracting major triad and minor triad into *ma* and *mì* respectively (with Italian vowels), and naming the three-cell triads in order from bottom to top, these 8 trichordals are distinguished as follows in the C decad. The triads are spread out and marked by + and −, and the terminal triads are repeated in part with an interposed | to indicate the dissonant interval of a Pythagorean minor Third= $\frac{80}{81} \cdot \frac{6}{5} = \frac{32}{27}$, so that all the harmonies, consonant and dissonant, peculiar to any trichordal, may be collected at a glance.

i. *Mamama*.........	B −D	F+ A −C+ E −G+ B −D	F+ A
ii. *Mimama*.........	B −D	F−†Ab+C+ E −G+ B −D	F−†Ab
iii. *Mamima*.........	B −D	F+ A −C−†Eb+G+ B −D	F+ A
iv. *Mimima*.........	B −D	F−†Ab+C−†Eb+G+ B −D	F−†Ab
v. *Mamami*.........	†Bb+D	F+ A −C+ E −G−†Bb+D	F+ A
vi. *Mimami*.........	†Bb+D	F−†Ab+C+ E −G−†Bb+D	F−†Ab
vii. *Mamimi*.........	†Bb+D	F+ A −C−†Eb+G−†Bb+D	F+ A
viii. *Mimimi*.........	†Bb+D	F−†Ab+C−†Eb+G−†Bb+D	F−†Ab

Each of these 8 trichordals contains 7 tones, and when these are reduced to one Octave and sounded in order of pitch, they form that particular scale in which a piece of music is usually written. But in repeating them each may begin on any tone of the seven, giving 7 *modes* (in the ancient Greek sense) to each trichordal. To distinguish these, change the *m* of the name of the triad containing the initial tone into *p* when it is its First (*p* rima), *t* when it is its Third (*t* ertia), and *qu* when it is its Fifth (*qu* inta), which last is of course required for the highest or dominant triad only. The final cadence fully distinguishes the 56 resulting *harmonic scales.* Of these I append such as are usually acknowledged, making them all begin with C, and changing the decad accordingly. Between the tones I use (.) for the Semitone $\frac{16}{15}$, (:) for the high Semitone $\frac{27}{25}$, (..) for the minor Tone $\frac{10}{9}$, (...) for the major Tone $\frac{9}{8}$, and (∴) for the augmented tone $\frac{75}{64}$.

1. C *mapáma*, or ordinary scale of C *major*.

c ... *d* .. *e* . *f* ... *g* .. *a* ... *b* . *c′*.

2. F *mamapá*, one of Helmholtz's modes of the Fourth, or *Quartengeschlecht*.

c .. ‡*d* ... *e* . *f* ... *g* .. *a* . *b*♭ ... *c′*.

3. C *mipáma*, Helmholtz's minor-major mode, or *Moll-Durgeschlecht.*

c ... *d* .. *e* . *f* ... *g* . †*a*♭ ∴ *b* . *c′*.

4. C *mapíma*, Helmholtz's mode of the minor Seventh with the leading note, or *Septimengeschlecht mit dem Leittone*, a very usual form of the modern ascending scale of C minor.

c . . . d . †e♭ . . f . . . g . . a . . . b . c′.

5. C *mipíma*, the theoretical modern ascending scale of C minor.

c . . . d . †e♭ . . f . . . g . †a♭ .·. b . c′.

6. F *mamapí*, considered by Helmholtz (*op. cit.* p. 434. no. 6) as a variant of the mode of the minor Seventh.

c . . ‡d : †e♭ . . f . . . g . . a . b♭ . . . c′.

7. C *mapími*, Helmholtz's mode of the minor Seventh without the leading note.

c . . . d . †e♭ . . f . . . g . . a : †b♭ . . c′.

8. C *mipími*, Helmholtz's mode of the minor Third, or *Terzengeschlecht*, the ordinary form of the modern descending scale of C minor.

c . . . d . †e♭ . . f . . . g . †a♭ . . . †b♭ . . c′.

9. F *mimipí*, Helmholtz's mode of the minor Sixth, or *Sextengeschlecht.*

c . d♭ . . . †e♭ . . f . . . g . †a♭ . . b♭ . . . c′.

These 56 harmonic scales are all that can be produced without modulation.

To retain old names as much as possible, C *mapáma* will be called C *major*, and all three, C *mipími*, *mipíma*, *mapíma*, will be considered as making up C *minor*, whilst other forms will be termed *unusual minor scales.* All these, however, and more of the 56 scales mentioned above, actually occur in modern music, at least for short phrases, although the usual *major* and *minor* alone characterize whole compositions.

D. *Modulation and Duodenation.*

Although a decad consists of complete triads and cells, yet it is evident that one or two of the cells may be made parts of other decads, and that the *union* triads may be regarded as parts of cells left incomplete. The tones forming these cells and unions are therefore *ambiguous*, and there is always a tendency to complete them in a different way from that in the original decad, or, in other words, to proceed to the other decads of which they form a part. By an extension of the term modulation, which originally referred to a mere change of mode, this change of decad might still be called *modulation*, although *decadation* might be more appropriate.

The Harmonic Heptadecad, or Unit of Modulation (or Decadation),

†d♭	†f	†a		
†g♭	†B♭	D	f♯	
†c♭	†E♭	G	B	‡d♯
†f♭	†A♭	C	E	‡g♯
	d♭	F	A	‡c♯
		b♭	‡d	‡f♯

or

ru	*fo*	*le*		
su	*To*	*Re*	*fi*	
du	*Mo*	*So*	*Ti*	*ri*
fu	*Lo*	*Do*	*Mi*	*se*
	ro	*Fa*	*La*	*de*
		ta	*ra*	*fe*

consists of seven interwoven decads, which are constructed on the seven tones of a heptad as tonics, and contains 24 tones. On the left is the heptadecad of C, in which the decad of C is printed in capitals, and the added tones necessary to complete the heptadecad in small letters. On the right *solfeggio* names are proposed as substitutes, to be pronounced with Italian vowels. These names are founded on those used by the Tonic Solfaists, and are suitable to *any* original tonic *Do*; and they are introduced because singers in just intonation should become accustomed to the "mental effect" of each of these tones in relation to the *Do* selected. The decads are named from the names of the tones in the original decad. The G decad is the dominant or *So* decad; the F the subdominant or *Fa* decad; the A the right relative, or major Sixth, or *La* decad; the †E♭ is the left relative or minor Third or *Mo* decad; the E is the right correlative or major Third, or *Mi* decad; and the †A♭ is the left correlative or minor Sixth or *Lo* decad—all with reference to the C or tonic or *Do* decad. These six decads are related to the original decad in the *first* degree. The dominant and subdominant decads have each *seven* tones, the relative and correlative decads have each *six* tones, in common with the original decad. The dominant decad raises two tones, F and A, by a comma, $\frac{81}{80}$, to †f and †a, and one, F, by a sharp, $\frac{135}{128}$, to f♯. The subdominant depresses two tones, †B♭ and D, by a comma, to b♭ and ‡d, and one, D, by a sharp, to d♭. These two decads are therefore equally related to the original. The right relative decad depresses one tone, D, by a comma, to ‡d, and raises three tones, F, C, G, by a low sharp, $\frac{25}{24}$, to ‡f♯, ‡c♯, ‡g♯. The right correlative decad raises one tone, F, by a sharp, to f♯, and three tones, C, G, D, by a low sharp, to ‡c♯, ‡g♯, ‡d♯. Hence the right relative is more nearly related to the original than the right correlative. Similarly the left relative, changing F in †f, and C, G, D into †c♭, †g♭, †d♭, is more nearly related than the left correlative, which changes D into d♭, and F, C, G into †f♭, †c♭, †g♭. In commatic temperaments, where ‡d, †f are not distinguished from D, F, the relative decads seem, like the just dominant and subdominant, to have 7 tones in common with the original, and similarly the dominant and subdominant decads appear to have 9 tones in common with the original. Hence various important confusions have arisen, of which it must suffice to have indicated the source. Since the most natural and easy harmonic tri-

chordal, with the fullest and best harmonies, is undoubtedly the *mamama* or *major*, consisting of the central and right columns of the decad containing it, modulation (or decadation) to the *right* is more common than modulation to the *left*; and, owing to the closer relationship, modulation to the right relative is more common than into the right correlative, which generally occurs as a *vertical* (dominant) modulation from the latter. Vertical (dominant or subdominant) modulations are, however, the most common of all, unconsciously (owing to commatic temperament) into the subdominant (when the minor chord, ‡d—f+a, is used for the chord of the added Sixth, f+a | d), and consciously into the dominant (in which, however, only f♯, and not †a, is commonly recognized).

The vertical modulation is so common that it influences scales, producing actual tetrachordals, which are disguised in melody by being occasionally deprived of their extreme tones, so as to reduce their apparent number at any time to 7. The fourth chord may be added on to the name by a hyphen. Thus we have

C *ma-mapáma*...... b♭+‡d—f+a—c+e—g+b—d,

in which the b♭ is seldom touched except in the chord of the dominant Seventh, c+e—g | b♭, and then not in melody, but ‡d often comes into melody. Similarly we have

C *mapáma-ma*...... f+a—c+e—g+b—d+f♯—†a,

where †a is not touched in the melody. But in minor scales this is more marked, as

A *mimipá-ma*...... ‡d—f+a—c+e+‡g♯—b+‡d♯—f♯,

where ‡d and f♯ are not touched in the melody; so that the scale reads

e . f ∴ *‡g♯ . a* ... *b . c′* ∴ *‡d′♯ . e′*,

with 4 semitones and 2 augmented tones, which has an extremely strange effect[1]. Another scale of this kind is

A *mimipí-ma*...... ‡d—f+a—c+e—g+b+‡d♯—f♯,

which occurs in the modern treatment of Helmholtz's mode of the minor Sixth (No. 9, above). The apparent scale is

e . f ... *g* .. *a* ... *b . c* ∴ *‡d′♯ . e′*,

which has 3 Semitones. These are, in fact, all cases of vertical modulation (or decadation); and it is only by recognizing this fact that we are able to reduce them to just intonation. They have not been, however, hitherto so conceived, and hence it became necessary, for the purposes of

[1] This scale and its harmonies are taken from C. Child Spenser's 'Rudimentary and Practical Treatise on Music,' vol. ii. p. 42. He does not acknowledge either ‡d or f♯; but he really uses ‡d in his second chord, *‡d a ‡d′ f′*, and he only avoids f♯ by using f+a... b+‡d♯ for the usual chord of the dominant Seventh, b+‡d♯—f♯ | a.

this paper, to explain them. A means of putting the strange-looking chords[1], f+a ... b+‡d♯, and f+a—c with ‡d♯, or f+a with ‡d♯, which they contain, under the hands of the performer on justly intoned instruments, is absolutely necessary.

The Harmonic Duodene or Element of Modulation, as distinguished from the heptadecad or unit of modulation, contains the 12 tones inclosed within an oblong in the figure of the heptadecad. It is seen to contain a complete decad of C and two additional tones, f♯ and d♭, which I term *mutators*, as each of them is part of two cells, and hence lead the old decad to *change* into the new decads containing them. Thus f♯ is part of

the vertical cell	†f †a	and of the lateral cell	D f♯	
	D f♯		B ‡d♯,	

and hence leads both to the dominant decad and to the right correlative decad. Again, d♭ is a part of

the vertical cell	d♭ F	and of the lateral cell	†f♭ †A♭
	b♭ ‡d		d♭ F,

and hence leads to the subdominant decad and to the left correlative decad. But these mutators, f♯ and d♭, also complete two scales left incomplete in the decad because they required vertical modulation (or decadation), namely,

†A♭ *mapáma* .... d♭+f —†a♭+c —†e♭+g—†b♭, and
E *mipími* .... a —c+ e —g+ b —d+ f♯,

and also complete the peculiar chords, d♭+F ... G+B, †A♭+C ... D+f♯, D♭+F—†A♭ with B, †A♭+C—†E♭ with f♯, which occur in the tetrachordals of minor scales already mentioned.

A *duodene*, then, consists of 12 tones, forming four *trines* of major Thirds arranged in three *quaternions* of Fifths. Hence the duodene constructed on the second tone C of any trine, †A♭+C+E, contains the *mapáma* or major of the first tone †A♭, the complete *decad* of the second tone C, and the *mipími* or common *descending* minor of the third tone E. It has therefore *three* tonics, †A♭, C, and E; but the tonic of the decad being most characteristic, this is called the *root* of the duodene, and the duodene is named after it.

Any duodene is clearly and sharply separated from its adjacent trines and quaternions, as shown in Table I., where the small innermost oblong marks off the duodene of C. For in the duodene the smallest intervals between two adjacent tones are the Semitone, $\frac{16}{15}$, (f♯ to G, B to C, E to F,

[1] The justification of these chords is that the interval *f* to ‡*d'*♯ $= 2 \cdot \frac{81}{80} \cdot \frac{9}{8} \cdot \frac{135}{128} \div \frac{4}{3} = \frac{7}{4} \cdot \frac{225}{224}$, and is hence very nearly the interval of the natural Seventh $= \frac{7}{4}$.

D to †E♭, G to †A♭, C to d♭); the Sharp, $\frac{135}{128}$, (F to f♯, d♭ to D); and the low Sharp, $\frac{25}{24}$, (†B♭ to B, †E♭ to E, †A♭ to A). But if we take the trine above, †f + †a + c♯, we have two intervals of a comma, †, (F to †f, A to †a), and one of a diaskhisma, †|♭, (c♯ to d♭). If we take the trine below, g♭ + b♭ + ‡d, we have the same intervals of a comma (b♭ to †B♭, ‡d to D), and a diaskhisma (f♯ to g♭). If we take the quaternion to the right, as ‡c♯ × ‡g♯ × ‡d♯ × a♯, we have three intervals of a diesis, ††|♭, (a♯ to †B♭, ‡d♯ to †E♭, ‡g♯ to †A♭, and ‡c♯ to d♭); and similarly if we proceed to the left. Hence the intervals introduced by adjacent trines and quaternions are all less than two commas. In equal temperament no new intervals would be thus introduced; for all the Fifths are there so altered that the new upper trine, tempered †f + †a + c♯, would become *identical* with the original bottom trine, tempered d♭ + F + A, except in order of terms; and the new quaternion to the right, tempered ‡c♯ × ‡g♯ × ‡d♯ × a♯, would be *identical* both in value and order of terms with the old quaternion to the left, tempered d♭ × †A♭ × †E♭ × †B♭. The consequence is that *only one duodene* exists for equal temperament, and the real nature of modulation is thoroughly disguised. In tertian temperament this would not be the case; the quaternions would be distinguished, but the trines would partly coincide, and hence some, but not all, of the meaning of modulation would be lost[1].

[1] If in Table I. the signs † ‡ be omitted, and the letters and the signs ♯ b be taken to have their values in Tertian or any uniform commatic temperament (except the Equal, which is also skhismatic), the Table will represent the corresponding duodenes. But if the letters and signs ♯ b are taken to have their value in the Equal temperament, so that

	C	D	E	F	G	A	B
	=Dbb	Ebb	Fb	Gbb	Abb	Bbb	Cb
and	=B♯	C♯♯	D♯♯	E♯	F♯♯	G♯♯	A♯♯

and

C♯	D♯	E♯	F♯	G♯	A♯	B♯
=Db	Eb	F	Gb	Ab	Bb	C

(showing the utterly absurd relations between symbolization and signification), then the same Table will reduce to the one central duodene with its tones differently distributed. This will be still better shown by using

C cd D de E F fg G ga A ab B

for the 12 digitals on a piano, so that the central duodene and its adjacent trines and quaternions reduce to

cd	F	A	cd	F
fg	ab	D	fg	ab
B	de	G	B	de
E	ga	C	E	ga
A	cd	F	A	cd
D	fg	ab	D	fg

In skhistic intonation, the modification of the Fifth leads to a modification of the comma and obliteration of the skhisma; so that the two first tones, skhistic †f, †a, of the new upper trine, †f+†a+c♯, are *one* skhistic comma *higher*, and the third, skhistic c♯, is *one* skhistic comma *lower* than the two last tones, F, A, and the first tone D♭ of the old trine, D♭+F+A. And the tones of the new right quaternion will be in the same order, exactly *two* skhistic commas *flatter* than the old left-hand quaternion[1].

The consequence is that if we took 4 *independent* duodenes (that is, such that no tone of one is common to any tone of the other) as the duodenes of †B♭, A♯, G♭, and ‡F♯, the tones of which are contained within the dotted lines and right side of the inner oblong of Table I., the tones of the duodenes A♯ and ‡F♯ will be two commas flatter than those of †B♭ and G♭; and the tones of the *two first* quaternions of the †B♭ and A♯ duodenes will be one comma *sharper* than those of the *two last* quaternions of G♭ and ‡F♯, while the tones of the *third* quaternions of †B♭ and A♯ will be one comma *flatter* than those of the duodenes of G♭ and ‡F♯ respectively.

The result, then, is that the 48 tones will consist of four corresponding sets of 12 tones each appearing in 4 forms, differing in pitch by one skhistic comma. This will appear more clearly by the following Table, in which the value in octs of the cycle of 301 is given for 73 tones, being those in cols. I. to VI. of Table I., less those in col. I., lines *l*, *m*, *n*, and col. VI., lines *y*, *z*. The 48 of those tones contained in 4 independent duodenes are in Roman capitals, the other of the 73 tones, which are some of their skhistic synonyms, are in Roman small letters, and other synonyms are added in *Italics*; the whole are divided into groups of 4, the constituents of which differ from one another by 5 octs, or one skhistic comma.

[1] This is readily seen by expressing the tones in terms of the octs of the cycle of 301, by continually adding and subtracting 176 for the Fifths and 97 for the major Thirds, adding or subtracting 301 as often as is necessary to reduce to the same Octave. A skhistic comma is represented by 5 octs. This gives

Tones.					*Octs.*				
†db	†f	†a	c♯	e♯	33	130	227	23	120
†gb	†Bb	D	F♯	a♯	158	255	51	148	245
†cb	†Eb	G	B	‡d♯	283	79	176	273	69
†fb	†Ab	C	E	‡g♯	107	204	0	97	194
†bbb	Db	F	A	‡c♯	232	28	125	222	18
†ebb	gb	bb	‡d	‡f♯	56	153	250	46	143

Tones.	Octs.	Tones.	Octs.	Tones.	Octs.
‡C# ‡‡db b##	18	‡E# ‡‡f ‡‡‡gbb	115	‡G## ‡a ‡‡bbb	217
C# ‡db †b##	23	E# ‡f ‡‡gbb	120	g## A ‡bbb	222
†c# Db ††b##	28	†e# F ‡gbb	125	†g## †A bbb	227
††c# †Db †††b##	33	††e# †F gbb	130	††g## ††a †Bbb	232
‡C## ‡‡d ‡‡ebb	41	‡F# ‡‡gb e##	143	‡A# ‡‡bb	240
c## ‡D ‡ebb	46	F# ‡gb †e##	148	A# ‡bb	245
†c## D ebb	51	†f# Gb ††e##	153	†a# Bb	250
††c## †D †ebb	56	††f# †Gb †††e##	158	††a# †Bb	255
‡‡D# ‡‡eb	64	‡F## ‡‡g ‡‡abb	166	a## ‡B ‡‡cb	268
‡D# ‡eb	69	f## ‡G ‡abb	171	†a## B ‡cb	273
d# Eb	74	†f## G abb	176	††a## †b Cb	278
†d# †Eb	79	††f## †g †Abb	181	†††a## ††b †Cb	283
‡D## ‡e ‡‡fb	92	‡G# ‡ab	194	‡B# ‡‡c ‡‡‡dbb	291
d## E ‡fb	97	G# ab	199	B# ‡c ‡‡dbb	296
†d## †E fb	102	†g# †Ab	204	†b# C ‡dbb	0
††d## ††e †Fb	107	††g# ††Ab	209	††b# †C dbb	5

Since, then, the duodene of C is precisely adapted for placing on our ordinary manuals, and no corresponding tones which have to be introduced within these limits will be more than two or three commas sharper or flatter than these, such corresponding tones (owing to our habits of reading musical notes into directions for using digitals) will be all fitted for being played on the same digitals. This is the most important point in the practical construction of instruments, and is for the first time pointed out in this paper.

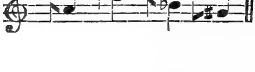
Another important result is, that if we take any 12 consecutive skhistic tones in order of Fifths, or 8 consecutive tones in that order, and 4 others separated from them by 24 or 48 Fifths, although such tones will not form a duodene, they will be 12 tones suitable for our manuals, and will therefore afford the means of temporarily supplementing other arrangements.

In skhistic intonation, then, the modulational peculiarities of just intonation are preserved; and it will be convenient in future to consider modulation as taking place by duodenes, and hence consisting of *duodenation*. We shall therefore have in just intonation both vertical and lateral duodenation to consider; but in skhistic intonation it will be seen by Table II. that *one right lateral duodenation*, as from root †B♭ to root D, is the same as *eight descending vertical duodenations*, for these would in just intonation lead to the root E♭♭, which, as shown in the last Table, is skhistically identical with D. Hence in skhistic intonation we have, so far as instruments are concerned, only to render vertical duodenation possible and easy.

And in writing music, if we note at the top of any bar the name of the duodene to which the notes to be played belong, and suppose this *duodenal* (as the mark may be called, in contradistinction to the *signature*, which will remain as before) to hold till a new one is written (according to the custom of musical signatures), we shall be able precisely to mark the pitch of every tone in just or skhistic intonation, *without introducing any change or any additional sign into the staff-notation of music*[1]. This is again an entirely new practical principle resulting from the present theory. The duodenal will direct the player to the mode of arranging the manual he has to use. It should be the duty of the composer to insert the duodenals himself; but in respect of existing compositions, which were composed for some commatic system of temperament, it will be often difficult to determine which of two adjacent vertical duodenes it would be best to use; and it will probably be necessary to introduce commatic changes, when they can be made within the limits of a single heptadecad. Also in the case of compositions in a *major* scale, which do not change into the minors of the same decad, and hence use only two quaternions of a duodene, but will necessarily and frequently modulate to the right, it is more convenient to consider the music as performed in the first and second quaternions of a duodene having for its root the Third of the major scale, because the third quaternion of that duodene contains the tones required for right lateral modulation. Thus C major will be assigned to the duodene of E, No. 19 of Table II., and F major to the duodene of A, No. 20 of Table II., &c. This makes the modulation from the major into the relative minor as simple and direct as vertical modulation, for C major passes into any form of A minor or major by descending vertically from the duodene of E to that of A. All pieces in any minor scale pass into each of the three quaternions of a duodene, and hence their duodenal will be the tonic of their decad, which gives its name to the duodene. The duodene is then prepared for playing the synonymous major of that minor scale. Such duodenals might be distinguished by an added star.

It often happens that passing tones, changing notes and *appoggiature*, are introduced which do not belong to the harmony. They are written usually after the laws of Pythagorean temperament, but their pitch is really indeterminate. For these there is no occasion to change the duodenal at all. They will then be played in the duodene of the other

[1] For theoretical and experimental purposes it may be sometimes convenient to use signs equivalent to † ‡ in the staff-notation itself. The signs  for †, ††, and  for ‡, ‡‡, being the tails of quavers and semiquavers, are well adapted for this purpose. The direction of the angles show ascent and descent, and the forms exist as types for every required position on the staff; thus †a, †d, †bb, and ‡g#, would be



harmonies by a tone of not more than two commas different, which must be considered as their proper representative in just intonation.

E. *Number of Tones required.*

The next important point is to determine how many duodenes must be provided. In Table I. the large *inner* oblong contains all the duodenes which have at least *one* tone in common with the original duodene of C. Thus the duodenes †D♭ and ‡B have respectively the tones †B♭ and A in common with the original duodene, and no others. If we proceeded further, as vertically from the †D♭ to the ††A♭ duodene, we should no longer have any connexion with the decad of the original tonic C. The confusions of modern equal temperament might lead much further; but in that case we must restore the commatic changes which equal temperament ignored, considering, for example, that when the composer modulated into tempered A♭ from tempered D♭ he really meant to make a modulation from just D♭ into just †A♭, and not from †D♭ to just ††A♭. It would probably have never been the composer's intention to proceed to such unrelated duodenes as these two last.

The limits of the *original roots* of duodenes may be taken to be the tones of the duodene of C. Practically, composers had no others in their minds. Any smaller changes of pitch were relegated to differences in the pitch of C, whence all the others were derived. If, then, we construct the limiting duodenes to the extreme tones of the duodene of C as original roots, we shall obtain all the tones in Table I., being $9 \times 13 = 117$ in number. This is the number of tones required, therefore, in just intonation.

Skhistic intonation would introduce identifications which would reduce this number to the $9 \times 8 = 72$ tones in the lines p to x in Table I., together with *three* in col. 1, lines l, m, n, and *two* in col. 9, lines y and z, that is to 77 skhistic tones in all. The last 5 are so extremely unlikely to occur, however, that we may consider these 72 skhistic tones as sufficiently representing the whole 117 of the Table. These 72 tones form 6 independent duodenes, those of ††E♭♭, D and ‡C♯♯, and of †C♭♭, B♭ and ‡A♯. It will be shown that there is really no difficulty in playing them all, with existing means, if required; but they would not be required. The tendency of musicians is not to modulate to both right and left equally in the same piece. It has been already noted that on account of the prevalence of major scales duodenation is generally to the right. The fingering for the duodenes of †B♭ and A♯ would be the same on manuals constructed on the duodenary theory, although the tones in skhistic intonation would differ by two skhistic commas. If, then, a piece in †B♭ duodenated much to the left, that is (for skhistic intonation), ascended vertically, we could play it as A♯. It would simply be necessary to write ‡B♭ as its duodenal, as that is shown to be identical with A♯ in the last Table. We should then be able to use ascending duodena-

tion with great ease, as shown in Table II., even if columns 1 and 2 in Table I. were omitted. Hence we may begin by cancelling columns 1 and 2 of Table II. In view of the greater frequency of right lateral or descending duodenation, we need only reject column 9 to the right[1]. We have thus reduced the just tones to the $6 \times 13 = 78$ in columns I. to VI. of Table I. The skhistic identifications reduce these further to the $6 \times 8 = 48$ tones in lines p to x of the same columns, together with the three tones in col. I., l, m, n, and the two in col. VI., y, z. As these tones may, I think, be always avoided by properly choosing the original root, motives of convenience induce me to reduce the number of skhistic tones necessary to the 48 included by the dotted lines in cols. I. to VI. of Table I.

In my former paper ('Proceedings,' vol. xiii. p. 98), not having taken a sufficiently comprehensive view of the nature of modulation, I fixed the number of just tones required at 72 instead of 117, and showed that they would reduce by skhismatic substitution (for I had not then worked out the theory of skhistic temperament) to 45; and on examination it will be found that these 45 include the 48 which I have just named, with the exception of those in col. I., lines p and q, and col. VI., line x of Table I. The tones used in Mr. Liston's organ (according to the statement I was able to give in 'Proceedings,' vol. xiii. p. 417, note §), on being treated skhistically, include 44 of these 48 tones, omitting the 4 tones in col. I., lines w, x, col. V., line x, and col. VI., line x, and introduces two others found in col. 2, lines p and x, and probably only due to his system of tuning. He has thus 46 tones in all. Gen. T. Perronet Thompson's organ (see my paper in 'Proceedings,' vol. xiii. p. 102), when similarly reduced, has 38 of my 48 tones, omitting all col. I., col. II., line x, and col. VI., line r in Table I., and retaining the two tones of col. VI., lines y, z, which I do not find necessary. Mr. Poole's latest organ (Silliman's Journal for 1867, pp. 1 to 45), after rejecting his 39 natural Sevenths, which I expressly exclude, has 61 just tones, which reduce to the 36 skhistic tones in col. II., lines q to x, cols. III., IV., and V., lines p to x, and col. VI., lines p to t. These are the principal attempts at limiting the scale actually made up to this time; and hence I conclude that my reduction to 48 skhistic tones (that is, practically, unequally just tones) would embrace almost every case, though it is conceivable that some extraordinary music might make it advisable to introduce 12 more, namely, col. 2, lines p to x, and col. 9, lines p to s, making 60 tones in all, for adding which provision should be made.

[1] If in changes of key in the movements of a long piece modulation took place first much to the left and then much to the right, we might perhaps make commatic changes between the movements, without disturbing the connexion. And when changes are introduced by successions of discords, such commatic changes could not be observed at all by the listener. By the use of the duodenal, however, they will be rendered perfectly simple to the performer.

The 48 tones thus pointed out form the 32 trines, which, with their skhistic synonyms, are shown on Table II. By taking these in quaternions we obtain 29 duodenes. In Table II. the root of the duodene is written against its uppermost trine, and hence the root itself is found in the middle of the second trine below, and the whole duodene extends to the third trine below. Trines on the same line are skhistically identical, the capitals indicating the names of the 48 selected tones of cols. I. to VI., lines p to x, in Table I. In Table III. the roots of these duodenes are arranged in 4 columns, of which each tone in the same line is skhistically identical; but, proceeding from left to right, each tone is, in just intonation, one skhisma flatter than the next adjacent tone on the right.

In Table III., also, the tones in each of the duodenes are written down in the order in which they would stand on a manual; but the skhistic identities of the central column of tones in the preceding Table of Octs (p. 19) have been used to give the same names to all the tones in one column, exclusive of the prefixed † and ‡. We thus see clearly that three new tones are introduced by each new successive duodene, two falling and one rising by a skhistic comma, as respects the tones they replace. We also see that each tone prevails through 4 consecutive duodenes, and that there are 4, and only 4, varieties of †, ‡ in each column.

This completes the *theory* of the construction of instruments, because the rest is properly the work of the mechanician, and consists simply in devising a method for bringing each of these 29 duodenes under the hand of the performer, when indicated by the duodenal. It will be sufficient here to point out a few experimental instruments, to suggest some practical forms, and to show that means of playing in just intonation with fixed tones already exist.

F. *Justly or Skhistically Intoned Instruments.*

1. *Just Concertina* (exhibited when this paper was read). The C concertina described in my former paper (Proceedings, vol. xiii. p. 104) contains the portion of a heptadecad shown in the margin—that is, the duodene of E with the exception of A♯, and the duodene of A with the exception of ‡F♯. It has the whole decad of E, and the major scales of F, C, G, E. I have found it a most useful instrument in all my experiments. Using capitals for white and small letters for black studs, its 14 notes are tuned thus :—

†A		
D	F♯	
G	B	‡D♯
C	E	‡G♯
F	A	‡C♯
B♭	‡D	

C ‡c♯, D ‡d, E ‡d♯, F f♯, G ‡g♯, †A a, B b♭.

†F		
†B♭	D	
†E♭	G	B
†A♭	C	E
D♭	F	A

2. *Just Harmonium* (exhibited when this paper was read). For ordinary lecture and illustrative purposes, this is the cheapest and best instrument. It contains the portion of a heptadecad shown in the margin, and hence contains the duodene of C with the exception of F♯, the whole decad of C, and the major scales of †E♭ and †A♭. The two last show vertical modulation. Again, †E♭ major or †A♭ major to C decad shows right lateral, and, inversely, C decad to †E♭ major or †A♭ major shows left lateral modulation. The F and †F show the influence of a comma, and the difference between the just triad, D♭+F−†A♭, and the Pythagorean triad, D♭ with †F and †A♭. Also in the key of †E♭ major the difference can be shown between the minor chord, F−†A♭+C, and the chord of the added Sixth, †A♭+C | †F. It also contains the German Sixth, D♭+F−†A♭ with B (which is a close imitation of the chord of the natural Seventh), the Italian Sixth, D♭+F with B, and the French Sixth, D♭+F ... G+B. Using capitals and small letters for the black and white keys, they are arranged thus :—

C d♭ D †e♭ E F †f G †a♭ A †b♭ B.

The †F is placed on the F♯ digital, and the fingering is normal in other respects. The five tones, d♭×†a♭×†e♭×†b♭×†f, will then enable us to play all Scotch and other music containing only five tones, in perfectly just intonation, on the black digitals, and to show that such music cannot be harmonized. The practical direction for tuning is, "tune the following 7 major chords without beats, putting †F on the F♯ digital, F A C, C E G, G B D ; D♭ F †A♭, †A♭ C †E♭, †E♭ G †B♭, †B♭ D †F." A four-Octave instrument was thus tuned in two hours.

3. *Hephtharmonium.* This requires two rows of vibrators and Mr. Saunders's "tilting action," already described. The vibrators are disposed thus :—

Back	†D♭	‡c♯	‡D	‡d♯	†F♭	†F	‡f♯	†G♭	‡g♯	‡A	b♭	†C♭
Front	C	d♭	D	†e♭	E	F	f♯	G	†a♭	A	†b♭	B.

This harmonium contains all the tones in a heptadecad (whence its name), and consequently illustrates every kind of modulation in the first degree, and becomes a most valuable instrument for the lecturer and teacher of singing. The front vibrators, to which correspond the digitals when the stops are all pushed in, contain the whole duodene of C, and the stops enable the player to exchange their tones for those in the six other decads. All lie on the usual digitals except †D♭ and †G♭, which are placed on the long white digitals of C and G, in place of the short black digitals next to their right, as these were wanted for ‡c♯ and ‡g♯. This makes a slight difference in the fingering of †E♭ minor and similar scales.

4. *Helmholtz's Harmonium* (Tonempfindungen, p. 496). Two sets of vibrators are tuned, the back set to the duodene of E♭ or D♯, No. 14, Tables II. and III., and the front set to duodene of ‡C♭ or B, No. 18, of the same. This instrument contains the eight trines, Nos. 14 to 21, and the five duodenes, Nos. 14 to 18, Table II. The "tilting action" produces a most useful experimental instrument, which is far easier to use than Helmholtz's own double manual instrument, because it has only one manual, and requires no alteration in ordinary fingering. For this purpose the stops may be reduced to four, each changing a trine instead of a single note.

5. *Guéroult's Harmonium* (Comptes Rendus, 1872, p. 1188). This, again, may be treated as the last by means of Mr. Saunders's "tilting action." Two sets of vibrators must be used, the back set tuned to duodene ‡D♭ or C♯, No. 16, and the front to ‡B♭♭ or A, No. 20, of Tables II. and III. M. Guéroult tuned the B♯ of duodene No. 16 as $B'\sharp = \frac{54638}{54675} B\sharp$, so as to make the combinational tone of G♯ and B'♯ the same as that of B'♯ and E♭, the other tones being tuned in just intonation from C. Omitting this as unnecessary, the instrument contains the eight trines, Nos. 16 to 23 of Table II., and the five duodenes, Nos. 16 to 20. M. Guéroult arranged the tones somewhat differently for two manuals.

6. *Duóni, Trióni, Quartóni, Quintóni, Sestóni.* The Russian horn-band which visited London some years ago, and produed great effects by each performer's playing a single tone only (and hence, probably, in just intonation), and the customs of hand-bell and church-bell ringers, who each play a single note in a melody, have suggested to me the use of *two, three, four, five,* or *six* harmoniums or pianofortes, indicated by the above names, for the purpose of playing in skhistic or unequally just intonation, by means of two, three, four, five, or six performers, among whom the tones are distributed. The *Duóni* are intended for two independent duodenes, as in the two last cases, the *Quartóni* for four such, playing the whole 48 tones, the *Sestóni* for six, in the almost impossible case of 72 tones being required. The *Trióni* supplement the *Duóni* by using 12 additional tones, forming consecutive Fifths, and hence not constituting a duodene, by which means the 36 tones of Mr. Poole's compass can be played. The *Quintóni* supplement the *Quartóni* in a similar manner; but the first 8 tones are those in col. 2, lines *p* to *x*, and the last 4 those in col. 9, lines *p* to *s* of Table II.—giving 60 tones on the whole, chosen so as to supplement without changing the arrangement of the *Quartóni*.

In each case separate harmoniums or pianos are used, with no change in existing mechanism or fingering, but only in intonation; so that the instruments could be obtained and tuned in unequally just intonation, as

already described, without difficulty, at a day's notice. The music is to be marked with the proper duodenals, and the duodenes thus indicated are to be transcribed separately, and divided into parts by transverse lines, corresponding to the tones existing on the different instruments. The copyist writes out a separate part for each performer (which had better have an indication of the complete harmony annexed), in which only those notes that belong to his own instrument are written. Thus, suppose that the duodene is F♯, and the lines show what tones lie on the instrument (I., II., III.), as in the margin. Suppose that the succession of chords *e♯ g♯ b′ c″♯*, *f♯ c′♯ a′♯ c″♯*, and *B ‡d′♯ f′♯ b′* has to be played. The tones will be distributed as in the margin. Considerable practice would be necessary to take up the notes truly at the right moment, but *there is no longer any instrumental or digitational difficulty in playing in just intonation.*

(II.) †E	G♯	B♯	
†A	C♯	E♯	
(I.) D	F♯	A♯	(III.)
G	B	‡D♯	

(I.)	*c″♯*, *b′*	*c″♯*, *c′♯*, *f♯*	*b′*, *f′♯*, *B*
(II.)	*g♯*	—	—
(III.)	*e♯*	*a′♯*	*‡d′♯*

Leaving *Duóni* aside as sufficiently indicated in the two last cases, and of only experimental interest, and *Sestóni* as practically not required, it will be enough to explain the tuning of *Trióni*, *Quartóni*, and *Quintóni*.

Trióni. Tune the three instruments thus :—

(I.)	C	‡d♭	D	†e♭	E	†F	‡g♭	G	†a♭	†A	†b♭	B
(II.)	‡C	d♭	‡D	e♭	†E	F	g♭	‡G	a♭	A	b♭	†B
(III.)	‡B♯	‡c♯	‡C♯♯	‡d♯	‡E	E♯	‡f♯	‡F♯♯	‡g♯	‡G♯♯	a♯	‡B

Then (I.) is in the duodene of G, No. 10, and (II.) in that of E♭, No. 14 of Tables II. and III.; and this readily gives the method of tuning them. (III.) consists of 12 tones forming consecutive Fifths from E♯ to ‡E, col. 7, lines *q* to *y*, and from ‡G♯♯ to ‡B♯, col. 8, lines *q* to *t* of Table I. For (III.) begin by tuning ‡G♯ a major Third without beats to E in (I.), and then work up to E♯ and down to ‡E by Fifths, verifying with the corresponding major Thirds below in (I.) and (II.). Then tune ‡B♯ a major Third above ‡G♯, and tune up by Fifths to ‡G♯♯, verifying by the major Thirds below, which lie all in (III.). By this the three instruments are completely in tune, and give the 17 duodenes, Nos. 10 to 26, Table II., containing Mr. Poole's scale of 36 tones.

Quartóni are much simpler, because they contain the four independent duodenes,

(I.) of †B♭, No. 1,	(III.) of A♯, No. 25,
(II.) of G♭, No. 5,	(IV.) of ‡F♯, No. 29

in Table III., where the corresponding lines give the tuning of each

instrument. The arrangement of tones is managed as before. Suppose, for example, we have to play the succession of chords *e g c′ g′*, *f f c′ a′*, *d g f′ b′*, and *c g e′ c′* in the duodene of C, this duodene would be written and the tones would be distributed among the four instruments as follows :—

(I.)	†B♭	D	F♯	(III.)
	†E♭	G	B	
(II.)	†A♭	C	E	(IV.)
	D♭	F	A	

(I.)	*g g′*	—	*g d*	*g*
(II.)	*c*	*c′ f*	*f′*	*c c′*
(III.)	—	—	*b′*	—
(IV.)	*e*	*a′*	—	*e′*

Quintóni have five instruments, (I.) to (IV.) being tuned as in *Quartóni*, and (V.) added when by some extraordinary vagaries of modulation more than 48 tones are needed. (V.) is tuned thus, where the synonyms show the meaning of the arrangement :—

(V.)	†D♭♭	‡b♯♯	††E♭♭	†f♭♭	††F♭	†G♭♭	‡e♯♯	††A♭♭	‡f♯♯♯	††B♭♭	†c♭♭	‡A♯♯
=	††C	‡‡c♯	††D	††e♭	†††E	††F	‡‡f♯	††G	‡‡g♯	†††A	††b♭	‡‡B

The tuning of (V.) is effected thus :—Tune ††A♭♭ as a major Third below †C♭ on (I.), and then work up by Fifths to ††F♭ and down to †F♭♭, verifying by the major Thirds above in (I.) and (II.). Then tune ‡A♯♯ as a major Third above ‡F♯♯ in (III.), and work up by Fifths to ‡F♯♯♯, verifying by the major Thirds below in (III.). The notation of the tones, though inevitable, is frightful; but the tuning is very simple, and the use of the duodenal leaves the old staff-notation unchanged. It is most probable that the fifth instrument would never be wanted.

7. *Great and Small Duodenary Harmonium.* Although the mode just explained places just intonation at the immediate command of three or four performers, yet it seems necessary to suggest a mode of putting all the 17 or 29 duodenes at the command of a single performer. I suggest the following for consideration. It seems practicable, but would doubtless require much mechanical treatment from harmonium-builders before it would act properly. It will be enough to indicate the form of the great duodenary.

Take four sets of vibrators, tuned as for *Quartóni*, and placed one behind the other, each opening with a separate valve connected with a digital. Sometimes two digitals will have to be connected with the same valve. Conceive the manual as a set of 29 "steps," with $\frac{3}{4}$-inch "tread" and $\frac{1}{4}$-inch "rise," the lowest step next the performer. Each step for the length of an Octave is divided into 12 digitals corresponding to the columns in Table III. The width of

the digitals to be as follows for No. 11 of Table III., in *eighths of an inch*:—

C	d♭	D	†e♭	E	F	‡g♭	G	†a♭	A	†b♭	B
5	3	5	3	5	5	3	5	3	5	3	5

The digitals corresponding to the small letters are to rise $\frac{1}{4}$ inch above the others and to be bevelled, so that they are $\frac{3}{8}$ inch wide at bottom, and $\frac{1}{4}$ inch wide at top. Each step is then a miniature finger-board in the ordinary arrangement. Whenever any note occurs in 4 consecutive steps, as shown by the cross lines in Table III., its 4 digitals are to be consolidated into one, so that, except in "steps" 1 to 3 and 26 to 29, the digitals will be practically 3 inches long. To show which digitals are consolidated, colour the low wide digitals alternately white and light red, and the high narrow digitals alternately light blue and light brown, distinctions of colour easily seen. To mark the duodene, draw a black line, $\frac{1}{4}$ inch broad, across the digital bearing the name of the duodene, and put a black circle of $\frac{1}{4}$ inch in diameter on the tonic of the major scale which it contains. The lines thus marked, together with the alternation of colour, will clearly distinguish each duodene.

The depth of this manual from front to back would be $21\frac{3}{4}$ inches, and the rise $7\frac{1}{4}$ inches; the width of an Octave from C to B is $6\frac{1}{4}$ inches, and from C to c is $6\frac{7}{8}$ inches. This last width is $7\frac{3}{8}$ inches on the piano; but as the hand would on the duodenary always have to dip between high digitals to strike Octaves of low digitals, it must be held more upright, and hence its span will be less. A manual of five Octaves and one note, C to c'''', will be $31\frac{7}{8}$ inches long. The number of movable digitals in each column of Table III. is 8, which open only 4 valves; this will necessitate coupling—the details resulting from Table III., which may be considered as a ground-plan of this manual[1].

[1] When this paper was read I mentioned that the 48 tones, making 29 duodenes, of Tables II. and III. could be played on Mr. Bosanquet's "generalized key-board," as exhibited to the Royal Society when his paper was read on January 30, 1873, with less difficulty in mechanism than by the plan I proposed (of which a model was exhibited), but that slightly new fingering would then be necessary; and also that the 72 tones of Table I., lines p to x, making 53 duodenes, might be played by the same arrangement on a manual not larger than that which I proposed for the 48 tones or 29 duodenes; and hence that the sole advantage of my scheme for a manual was its preservation of the present fingering, against which had to be set off the advantage that the new fingering of Mr. Bosanquet would be the same in all keys or duodenes. The intonation, however, would remain different from Mr. Bosanquet's.

Table I.

Limits of Duodenation and Number of Tones.

			I.	II.	III.	IV.	V.	VI.		
	1.	2.	3.	4.	5.	6.	7.	8.	9.	
l	†††Bbb	††Db	††F	††A	†C♯	†E♯	G♯♯	B♯♯	‡D♯♯♯	*l*
m	†††Ebb	††Gb	††Bb	†D	†F♯	†A♯	C♯♯	E♯♯	‡G♯♯♯	*m*
n	†††Abb	††Cb	††Eb	†G	†B	D♯	F♯♯	A♯♯	‡C♯♯♯	*n*
p	††Dbb	††Fb	††Ab	†C	†E	G♯	B♯	‡D♯♯	‡F♯♯♯	*p*
q	††Gbb	††Bbb	†Db	†F	†A	C♯	E♯	‡G♯♯	‡B♯♯	*q*
r	††Cbb	††Ebb	†Gb	†Bb	D	F♯	A♯	‡C♯♯	‡E♯♯	*r*
s	††Fbb	††Abb	†Cb	†Eb	G	B	‡D♯	‡F♯♯	‡A♯♯	*s*
t	††Bbbb	†Dbb	†Fb	†Ab	C	E	‡G♯	‡B♯	‡‡D♯♯	*t*
u	††Ebbb	†Gbb	†Bbb	Db	F	A	‡C♯	‡E♯	‡‡G♯♯	*u*
w	††Abbb	†Cbb	†Ebb	Gb	Bb	‡D	‡F♯	‡A♯	‡‡C♯♯	*w*
x	†Dbbb	†Fbb	†Abb	Cb	Eb	‡G	‡B	‡‡D♯	‡‡F♯♯	*x*
y	†Gbbb	†Bbbb	Dbb	Fb	Ab	‡C	‡E	‡‡G♯	‡‡B♯	*y*
z	†Cbbb	†Ebbb	Gbb	Bbb	‡Db	‡F	‡A	‡‡C♯	‡‡E♯	*z*

The tones in the *small* central oblong form the duodene of which C is the root.

The tones in the *large* central oblong form all the duodenes which have at least one tone in common with the central duodene of C, forming the limits of radical duodenation from C.

The complete Table contains all the duodenes which have at least one tone in common with duodenes whose roots are tones in the duodene of C, forming the limits of general radical duodenation.

The 48 tones in columns I. to VI., between the dotted lines, are those considered sufficient for instruments with fixed tones in skhistic or unequally just intonation.

Table II.

List of Trines and Duodenes in order of Fifths.

The Capitals point out the 48 tones in Table I.; the small letters are synonyms.

Name.	No.	Trine.			Name.	No.	Trine.			Name.	No.	Trine.		
†Bb	1	††Ab	†C	†E	††A#	1	††g#	††b#	†d##					
†Eb	2	†Db	†F	†A	†D#	2	††c#	††e#	†g##					
†Ab	3	†Gb	†Bb	D	†G#	3	††f#	††a#	†c##					
Db	4	†Cb	†Eb	G	†C#	4	††b	†d#	†f##					
Gb	5	†Fb	†Ab	C	†F#	5	††c	†g#	†b#					
Cb	6	†Bbb	Db	F	†B	6	††a	†c#	†e#					
Fb	7	†Ebb	Gb	Bb	†E	7	†d	†f#	†a#					
Bbb	8	†Abb	Cb	Eb	†A	8	†g	†b	d#					
Ebb	9	dbb	fb	ab	D	9	†C	†E	G#	†C##	9	††b#	†d##	†f###
Abb	10	gbb	bbb	‡db	G	10	†F	†A	C#	†F##	10	††e#	†g##	†b##
‡Dbb	11	cbb	ebb	‡gb	C	11	†Bb	D	F#	†B#	11	††a#	†c##	†e##
‡Gbb	12	fbb	abb	‡cb	F	12	†Eb	G	B	†E#	12	†d#	†f##	†a##
‡Cbb	13	bbbb	‡dbb	‡fb	Bb	13	†Ab	C	E	†A#	13	†g#	†b#	d##
	14	ebbb	‡gbb	‡bbb	Eb	14	Db	F	A	D#	14	†c#	†e#	g##
	15	abbb	‡cbb	‡ebb	Ab	15	Gb	Bb	‡D	G#	15	†f#	†a#	c##
	16	‡dbbb	‡fbb	‡abb	‡Db	16	Cb	Eb	‡G	C#	16	†b	d#	f##
†E##	17	†d##	†f###	†a###	‡Gb	17	fb	ab	‡c	F#	17	†E	G#	B#
†A##	18	†g##	†b##	d###	‡Cb	18	bbb	‡db	‡f	B	18	†A	C#	E#
D##	19	†c##	†e##	g###	‡Fb	19	ebb	‡gb	‡bb	E	19	D	F#	A#
G##	20	†f##	†a##	c###	‡Bbb	20	abb	‡cb	‡eb	A	20	G	B	‡D#
C##	21	†b#	d##	f###	‡Ebb	21	‡dbb	‡fb	‡ab	‡D	21	C	E	‡G#
F##	22	†e#	g##	b##		22	‡gbb	‡bbb	‡‡db	‡G	22	F	A	‡C#
B#	23	†a#	c##	e##		23	‡cbb	‡ebb	‡‡gb	‡C	23	Bb	‡D	‡F#
E#	24	d#	f##	a##		24	‡fbb	‡abb	‡‡cb	‡F	24	Eb	‡G	‡B
A#	25	G#	B#	‡D##						‡Bb	25	ab	‡c	‡e
‡D#	26	C#	E#	‡G##						‡Eb	26	‡db	‡f	‡a
‡G#	27	F#	A#	‡C##						‡Ab	27	‡gb	‡bb	‡‡d
‡C#	28	B	‡D#	‡F##						‡‡Db	28	‡cb	‡eb	‡‡g
‡F#	29	E	‡G#	‡B#						‡‡Gb	29	‡fb	‡ab	‡‡c
	30	A	‡C#	‡E#							30	‡bbb	‡‡db	‡‡f
	31	‡D	‡F#	‡A#							31	‡ebb	‡‡gb	‡‡bb
	32	‡G	B	‡‡D#							32	‡abb	‡‡cb	‡‡eb

Table III.

Manuals for Duodenary Instruments.

The Capital letters indicate broad and low, small letters narrow and high, digitals.

Names of Duodenes with Synonyms.	No.	Digitals, containing the tones of the Duodenes displayed horizontally.											
		1	2	3	4	5	6	7	8	9	10	11	12
†Bb ††a#	1	†C	†db	D	†eb	†E	†F	†gb	G	††ab	†A	†bb	††B
†Eb †d#	2	C	†db	D	†eb	††E	†F	†gb	G	†ab	†A	†bb	††B
†Ab †g#	3	C	db	D	†eb	††E	F	†gb	G	†ab	††A	†bb	††B
Db †c#	4	C	db	†D	†eb	††E	F	gb	G	†ab	††A	bb	††B
Gb †f#	5	C	db	†D	eb	††E	F	gb	†G	†ab	††A	bb	†B
Cb †b	6	†C	db	†D	eb	†E	F	gb	†G	ab	††A	bb	†B
Fb †e	7	†C	‡db	†D	eb	†E	†F	gb	†G	ab	†A	bb	†B
Bbb †a	8	†C	‡db	D	eb	†E	†F	‡gb	†G	ab	†A	†bb	†B
ebb D †c##	9	†C	‡db	D	†eb	†E	†F	‡gb	G	ab	†A	†bb	B
abb G †f##	10	C	‡db	D	†eb	E	†F	‡gb	G	†ab	†A	†bb	B
‡dbb C †b#	11	C	db	D	†eb	E	F	‡gb	G	†ab	A	†bb	B
‡gbb F †e#	12	C	db	‡D	†eb	E	F	gb	G	†ab	A	bb	B
‡cbb Bb †a#	13	C	db	‡D	eb	E	F	gb	‡G	†ab	A	bb	†B
Eb d#	14	‡C	db	‡D	eb	†E	F	gb	‡G	ab	A	bb	†B
Ab g#	15	‡C	‡db	‡D	eb	†E	‡F	gb	‡G	ab	†A	bb	†B
‡Db c#	16	‡C	‡db	D	eb	†E	‡F	‡gb	‡G	ab	†A	‡bb	†B
‡gb F# †e##	17	‡C	‡db	D	‡eb	†E	‡F	‡gb	G	ab	†A	‡bb	B
‡cb B †a##	18	C	‡db	D	‡eb	E	‡F	‡gb	G	‡ab	†A	‡bb	B
‡fb E d##	19	C	‡‡db	D	‡eb	E	F	‡gb	G	‡ab	A	‡bb	B
‡bbb A g##	20	C	‡‡db	‡D	‡eb	E	F	‡‡gb	G	‡ab	A	bb	B
‡ebb ‡D c##	21	C	‡‡db	‡D	eb	E	F	‡‡gb	‡G	‡ab	A	bb	‡B
‡G f##	22	‡C	‡‡db	‡D	eb	‡E	F	‡‡gb	‡G	ab	A	bb	‡B
‡C b#	23	‡C	‡db	‡D	eb	‡E	‡F	‡‡gb	‡G	ab	‡A	bb	‡B
‡F e#	24	‡C	‡db	‡‡D	eb	‡E	‡F	‡gb	‡G	ab	‡A	‡bb	‡B
‡bb A#	25	‡C	‡db	‡‡D	‡eb	‡E	‡F	‡gb	‡‡G	ab	‡A	‡bb	B
‡eb ‡D#	26	‡‡C	‡db	‡‡D	‡eb	E	‡F	‡gb	‡‡G	‡ab	‡A	‡bb	B
‡‡ab ‡G#	27	‡‡C	‡‡db	‡‡D	‡eb	E	‡‡F	‡gb	‡‡G	‡ab	A	‡bb	B
‡‡db ‡C#	28	‡‡C	‡‡db	‡D	‡eb	E	‡‡F	‡‡gb	‡‡G	‡ab	A	‡‡bb	B
‡‡gb ‡F#	29	‡‡C	‡‡db	‡D	‡‡eb	E	‡‡F	‡‡gb	‡G	‡ab	A	‡‡bb	‡B

November 26, 1874.

W. SPOTTISWOODE, M.A., Treasurer and Vice-President, in the Chair.

In pursuance of the Statutes, notice was given from the Chair of the ensuing Anniversary Meeting, and the list of Officers and Council proposed for election was read as follows:—

President.—Joseph Dalton Hooker, C.B., M.D., D.C.L., LL.D.

Treasurer.—William Spottiswoode, M.A., LL.D.

Secretaries.— { Prof. George Gabriel Stokes, M.A., D.C.L., LL.D.
Prof. Thomas Henry Huxley, LL.D., Ph.D.

Foreign Secretary.—Prof. Alexander William Williamson, Ph.D.

Other Members of the Council.—Prof. J. Couch Adams, LL.D.; the Duke of Devonshire, K.G., D.C.L.; Capt. Frederick J. O. Evans, R.N., C.B.; John Evans, Pres. G.S., F.S.A.; Albert C. L. G. Günther, M.A., M.D.; Daniel Hanbury, Treas. L.S.; Sir John Hawkshaw, Knt., M.I.C.E.; Joseph Norman Lockyer, F.R.A.S.; Robert Mallet, C.E., M.R.I.A.; Nevil Story Maskelyne, M.A.; C. Watkins Merrifield, Hon. Sec. I.N.A.; Prof. Edmund A. Parkes, M.D.; Right Hon. Lyon Playfair, C.B., LL.D.; Andrew Crombie Ramsay, LL.D.; Major-Gen. Sir H. C. Rawlinson, K.C.B.; J. S. Burdon Sanderson, M.D.

The Presents received were laid on the table, and thanks ordered for them.

The following Paper was read:—

"Preliminary Notes on the Nature of the Sea-bottom procured by the Soundings of H.M.S. 'Challenger' during her Cruise in the 'Southern Sea' in the early part of the year 1874." By Professor C. WYVILLE THOMSON, F.R.S., Director of the Civilian Scientific Staff on Board. Received Nov. 12, 1874.

[Plates I.—IV.]

During our southern cruise the sounding-lead brought up five absolutely distinct kinds of sea-bottom, without taking into account the rock and detritus of shallow soundings in the neighbourhood of land. Our first two soundings in 98 and 150 fathoms, on the 17th and 18th of December, were in the region of the Agulhas current. These soundings would have been naturally logged "greenish sand;" but on examining the sandy particles with the microscope, they were found to consist almost without exception of the casts of Foraminifera in one of the complex silicates of alumina, iron, and potash, probably some form of glauconite.

The genera principally represented by these casts were *Miliola*, *Biloculina*, *Uvigerina*, *Planorbulina*, *Rotalia*, *Textularia*, *Bulimina*, and *Nummulina*; *Globigerina*, *Orbulina*, and *Pulvinulina* were present, but not nearly in so great abundance. There were very few Foraminifera on the surface of the sea at the time. This kind of bottom has been met with once or twice before; but it is evidently exceptional, depending upon some peculiar local conditions.

From the Cape, as far south as our station in lat. 46° 16′, we found no depth greater than 1900 fathoms, and the bottom was, in every case, "*Globigerina*-ooze;" that is to say, it consisted of little else than the shells of *Globigerina*, whole, or more or less broken up, with a small proportion of the shells of *Pulvinulina* and of *Orbulina*, and the spines and tests of Radiolarians and fragments of the spicules of Sponges.

Since the time of our departure, Mr. Murray has been paying the closest attention to the question of the origin of this calcareous formation, which is of so great interest and importance on account of its anomalous character and its enormous extension. Very early in the voyage, he formed the opinion that all the organisms entering into its composition at the bottom are dead, and that all of them live abundantly at the surface and at intermediate depths, over the *Globigerina*-ooze area, the ooze being formed by the subsiding of these shells to the bottom after death.

This is by no means a new view. It was advocated by the late Professor Bailey, of West Point, shortly after the discovery, by means of Lieutenant Brooke's ingenious sounding-instrument, that such a formation had a wide extension in the Atlantic. Johannes Mueller, Count Pourtales, Krohn, and Max-Schultze observed *Globigerina* and *Orbulina* living on the surface; and Ernst Haeckel, in his important work upon the Radiolaria, remarks "that we often find upon, and carried along by the floating pieces of sea-weed which are so frequently met with in all seas, Foraminifera as well as other animal forms which habitually live at the bottom. However, setting aside these accidental instances, certain Foraminifera, particularly in their younger stages, occur in some localities so constantly, and in such numbers, floating on the surface of the sea, that the suspicion seems justifiable that they possess, at all events at a certain period of their existence, a pelagic mode of life, differing in this respect from most of the remainder of their class. Thus Müller often found in the contents of the surface-net off the coast of France, the young of *Rotalia*, but more particularly *Globigerinæ* and *Orbulinæ*, the two latter frequently covered with fine calcareous tubes, prolongations of the borders of the fine pores through which the pseudopodia protrude through the shell. I took similar *Globigerinæ* and *Orbulinæ* almost daily in a fine net at Messina, often in great numbers, particularly in February. Often the shell was covered with a whole forest of extremely long and delicate calcareous tubes projecting from all sides, and probably contributing essentially to enable these little animals to float below the surface

of the water by greatly increasing their surface, and consequently their friction against the water, and rendering it more difficult for them to sink"*. In 1865 and 1866 two papers were read by Major Owen, F.L.S., before the Linnean Society, "On the Surface-fauna of Mid Ocean." In these communications the author stated that he had taken foraminifera of the genera *Globigerina* and *Pulvinulina* living, in the tow-net on the surface, at many stations in the Indian and Atlantic Oceans. He described the special forms of these genera which were most common, and gave an interesting account of their habits, proposing for a family which should include *Globigerina*, with *Orbulina* as a subgenus, and *Pulvinulina*, the name Colymbitæ, from the circumstance that, like the Radiolaria, these Foraminifera are found on the surface after sunset, "diving" to some depth beneath it during the heat of the day. Our colleague, Mr. Gwyn Jeffreys, chiefly on the strength of Major Owen's papers, maintained that certain Foraminifera were surface-animals, in opposition to Dr. Carpenter and myself†. I had formed and expressed a very strong opinion on the matter. It seemed to me that the evidence was conclusive that the Foraminifera which formed the *Globigerina*-ooze lived on the bottom, and that the occurrence of individuals on the surface was accidental and exceptional; but after going into the thing carefully, and considering the mass of evidence which has been accumulated by Mr. Murray, I now admit that I was in error; and I agree with him that it may be taken as proved, that all the materials of such deposits (with the exception of course of the remains of animals which we now know to live at the bottom at all depths, and which occur in the deposit as foreign bodies) are derived from the surface.

Mr. Murray has combined with a careful examination of the soundings, a constant use of the tow-net, usually at the surface, but also at depths from ten to one hundred fathoms; and he finds the closest relation to exist between the surface-fauna of any particular locality and the deposit which is taking place at the bottom. In all seas, from the equator to the polar ice, the tow-net contains *Globigerinæ*. They are more abundant, and of a larger size, in warmer seas; several varieties attaining a large size, and presenting marked varietal characters, are found in the intertropical area of the Atlantic. In the latitude of Kerguelen they are less numerous and smaller, while further south they are still more dwarfed, and only one variety, the typical *Globigerina bulloides*, is represented. The living *Globigerinæ* from the tow-net are singularly different in appearance from the dead shells we find at the bottom (Plate I.). The shell is clear

* Die Radiolarien. Eine Monographie von Dr. Ernst Haeckel. Berlin, 1862, page 166–167.

† "Mr. Jeffreys desires to record his dissent from this conclusion, since (from his own observations, as well as those of Major Owen and Lieutenant Palmer) he believes *Globigerina* to be exclusively an *Oceanic* Foraminifer inhabiting only the superficial stratum of the sea."—"Preliminary Report of the Scientific Exploration of the Deep Sea," Proceedings of the Royal Society, No. 121, page 443.

and transparent, and each of the pores which penetrate it is surrounded by a raised crest, the crest round adjacent pores coalescing into a roughly hexagonal network, so that the pore appears to lie at the bottom of an hexagonal pit. At each angle of this hexagon the crest gives off a delicate flexible calcareous spine, which is sometimes four or five times the diameter of the shell in length. The spines radiate symmetrically from the direction of the centre of each chamber of the shell, and the sheaves of long transparent needles, crossing one another in different directions, have a very beautiful effect. The smaller inner chambers of the shell are entirely filled with an orange-yellow granular sarcode; and the large terminal chamber usually contains only a small irregular mass, or two or three small masses run together, of the same yellow sarcode stuck against one side, the remainder of the chamber being empty. No definite arrangement, and no approach to structure, was observed in the sarcode, and no differentiation, with the exception of round bright-yellow oil-globules, very much like those found in some of the Radiolarians, which are scattered apparently irregularly in the sarcode. We never have been able to detect the least trace of pseudopodia in any of the large number of *Globigerinæ* which we have examined, nor any extension, in any form, of the sarcode beyond the shell.

Major Owen (*op. cit.*) has referred the *Globigerina* with spines to a distinct species, under the name of *G. hirsuta*. I am inclined rather to believe that all *Globigerinæ* are, to a greater or less degree, spiny, when the shell has attained its full development. In specimens taken with the tow-net the spines are very usually absent; but that is probably on account of their extreme tenuity; they are broken off by the slightest touch. In fresh examples from the surface the dots indicating the origin of the lost spines may almost always be made out with a high power. There never are spines on the *Globigerinæ* from the bottom, even in the shallowest water. Two or three very marked varieties of *Globigerina* occur; but I certainly do not think that the characters of any of them can be regarded as of specific value.

There is still a good deal of obscurity about the nature of *Orbulina universa*, an organism which occurs in some places in large proportion in the *Globigerina*-ooze. The shell of *Orbulina* (Pl. II.) is spherical, usually about ·5 millimetre in diameter, but it is found of all smaller sizes. The texture of the mature shell resembles closely that of *Globigerina*, but it differs in some important particulars. The pores are markedly of two different sizes, the larger about four times the area of the smaller. The larger pores are the less numerous; they are scattered over the surface of the shell without any appearance of regularity; the smaller pores occupy the spaces between the larger. The crests between the pores are much less regular in *Orbulina* than they are in *Globigerina*; and the spines, which are of great length and extreme tenuity, seem rather to arise abruptly from the top of scattered papillæ than to mark the intersections of the crests.

This origin of the spines from the papillæ can be well seen with a moderate power on the periphery of the sphere. The spines are hollow and flexible; they naturally radiate regularly from the direction of the centre of the sphere; but in specimens which have been placed under the microscope with the greatest care, they are usually entangled together in twisted bundles. They are so fragile that the weight of the shell itself, rolling about with the motion of the ship, is usually sufficient to break off the whole of the spines and leave only the papillæ projecting from its surface, in the course of a few minutes. In some examples, either those in process of development, or a series showing a varietal divergence from the ordinary type, the shell is very thin and almost perfectly smooth, with neither papillæ nor spines, nor any visible structure, except the two classes of pores, which are constant.

The chamber of *Orbulina* is often almost empty; even in the case of examples from the surface, which appear, from the freshness and transparency of the shell, to be living, it is never full of sarcode; but it frequently contains a small quantity of yellow sarcode stuck against one side, as in the last chamber of *Globigerina.* Sometimes, but by no means constantly, within the chamber of *Orbulina* there is a little chain of three or four small chambers singularly resembling in form, in proportion, and in sculpture a small *Globigerina*; and sometimes, but again by no means constantly, spines are developed on the surface of the calcareous walls of these inner chambers, like those on the test of *Globigerina.* The spines radiate from the position of the centre of the chambers and abut against the insides of the wall of the *Orbulina* (Pl. II.). In a few cases, the inner chambers have been observed, apparently arising within or amidst the sarcode adhering to the wall of the *Orbulina.*

Major Owen regards *Orbulina* as a distinct organism, nearly allied to *Globigerina,* but differing so far from it as to justify its separation into a special subgenus. He considers the small inner chamber of *Orbulina* to represent the smaller chamber of *Globigerina,* and the outer wall as the equivalent of the large outer chamber of *Globigerina,* developed in this form as an investing chamber. Count Pourtales, Max-Schültze, and Krohn, on the other hand, believe, on account of the close resemblance in structure between the two shells, their constant association, and the undoubted fact that an object closely resembling a young *Globigerina* is often found within *Orbulina,* that the latter is simply a special reproductive chamber budded from the former, and capable of existing independently. I am rather inclined to the latter view, although I think much careful observation is still required to substantiate it; and some, even of our own, observations would seem to tell somewhat in the opposite direction. Although *Orbulina* and *Globigerina* are very usually associated, they are so in different proportions in different localities; and in the icy sea to the south of Kerguelen, although *Globigerina* was constantly taken in the surface-net, not a single *Orbulina* was detected. Like

Globigerina, *Orbulina* is most fully developed and most abundant in the warmer seas.

Associated with these forms, and, like them, living on the surface, and dead, with their shells in various stages of decay, at the bottom, there are two very marked species or varieties of *Pulvinulina*, *P. Menardii*, and *P. Micheliniana*. The general structure of *Pulvinulina* resembles that of *Globigerina*. The shell consists of a congeries of from five to eight chambers arranged in an irregular spiral. As in *Globigerina*, the last chamber is the largest; the inner smaller chambers are usually filled with yellow sarcode; and, as in *Globigerina*, the last chamber is frequently nearly empty, a small irregular mass of sarcode only occupying a part of the cavity. The walls of the chambers are closely and minutely perforated. The external surface of the wall is nearly smooth, and no trace of a spine has ever been detected. *Pulvinulina Menardii* (Pl. III. fig. 1) has a large discoidal depressed shell, consisting of a series of flat chambers overlapping one another, like a number of coins laid down somewhat irregularly, but generally in a spiral; each chamber is bordered by a distinct somewhat thickened solid rim of definite width. On the lower surface of the shell the intervals between the chambers are indicated by deep grooves. The large irregular opening of the final chamber is protected by a crescentic lip, which in some specimens bears a fringe of spine-like papillæ. This form is almost confined to the warmer seas. It is very abundant on the surface, and still more so during the day, at a depth of ten to twenty fathoms in the Mid-Atlantic; and it enters into the composition of the very characteristic "*Globigerina*-ooze" of the "Dolphine Rise" in almost as large proportions as *Globigerina*. *Pulvinulina Micheliniana* is a smaller variety; the upper surface of the shell is flattened as in *P. Menardii*, but the chambers are conical and prolonged downwards, so that the shell is deeper and somewhat turbinate. The two species usually occur together; but *P. Micheliniana* has apparently a much wider distribution than *P. Menardii*; for while the latter was limited to the region of the trade-winds and the equatorial drift current, and was found rarely if at all to the south of the Agulhas current, the former accompanied us southward as far as Kerguelen Land. Both forms of *Pulvinulina*, however, are more restricted than *Globigerina*; for even *P. Micheliniana* became scarce after leaving the Cape, and the wonderfully pure calcareous formation in the neighbourhood of Prince Edward Island and the Crozets consists almost solely of *Globigerina bulloides*, and neither species of *Pulvinulina* occurred to the south of Kerguelen Land.

Over a very large part of the "*Globigerina*-ooze" area, and especially in those intertropical regions in which the formation is most characteristically developed, although the great bulk of the ooze is made up of entire shells and fragments of shells of the above-described foraminifera, there is frequently a considerable proportion (amounting in

some cases to about twenty per cent.) of fine granular matter, which fills the shells and the interstices between them, and forms a kind of matrix or cement. This granular substance is, like the shells, calcareous, disappearing in weak acid to a small insoluble residue; with a low microscopic power it appears amorphous, and it is likely to be regarded, at first sight, as a paste made up of the ultimate calcareous particles of the disintegrated shells; but under a higher power it is found to consist almost entirely of "coccoliths" and "rhabdoliths." I need scarcely enter here into a detailed description of these singular bodies, which have already been carefully studied by Huxley, Sorby, Gümbel, Haeckel, Carter, Oscar Schmidt, Wallich, and others. I need only state that I believe our observations have placed it beyond a doubt that the "coccoliths" are the separated elements of a peculiar calcareous armature which covers certain spherical bodies (the "coccospheres" of Dr. Wallich). The rhabdoliths are the like elements of the armature of extremely beautiful little bodies, of which two forms are represented in Pl. III. figs. 3 & 4, which have been first observed by Mr. Murray and naturally called by him "rhabdospheres." Coccospheres and rhabdospheres live abundantly on the surface, especially in warmer seas. If a bucket of water be allowed to stand over night with a few pieces of thread in it, on examining the threads carefully many examples may usually be found attached to them; but Mr. Murray has found an unfailing supply of all forms in the stomachs of *Salpæ*.

What these coccospheres and rhabdospheres are, we are not yet in a position to say with certainty; but our strong impression is that they are either Algæ of a peculiar form, or the reproductive gemmules or the sporangia of some minute organism, probably an Alga; in which latter case the coccoliths and rhabdoliths might be regarded as representing in position and function the "amphidisci" on the surface of the gemmules of *Spongilla*, or the spiny facets on the zygospores of many of the Desmideæ. There are many forms of coccoliths and rhabdoliths, and many of these are so distinct that they evidently indicate different species. Mr. Murray believes, however, that only one form is met with on one sphere; and that, in order to produce the numerous forms figured by Haeckel and Oscar Schmidt, all of which, and many additional varieties, he has observed, the spheres must vary in age and development, or in kind. Their constant presence in the surface-net, in surface-water drawn in a bucket, and in the stomachs of surface-animals, sufficiently proves that, like the ooze-forming Foraminifera, the coccoliths and rhabdoliths, which enter so largely into the composition of the recent deep-sea calcareous formations, live on the surface and at intermediate depths, and sink to the bottom after death. Coccospheres and rhabdospheres have a very wide, but not an unlimited, distribution. From the Cape of Good Hope they rapidly decreased in number on the surface and at the bottom, as we progressed southwards. The proportion of their remains in the *Globigerina*-ooze near the Crozets and Prince Edward Island was com-

paratively small; and to this circumstance the extreme clearness and the unusual appearance of being composed of *Globigerinæ* alone was probably mainly due. We found the same kind of ooze, nearly free from coccoliths and rhabdoliths, in what may be considered about a corresponding latitude in the north, to the west of Faröe.

Before leaving the subject of the modern Chalk, it may be convenient to pass on to stations 158, 159, and 160, on March 7th, 10th, and 13th, on our return voyage from the ice. The first two of these, at depths of 1800 and 2150 fathoms respectively, are marked on the chart "*Globigerina*-ooze;" and it will be observed that these soundings nearly correspond in latitude with the like belt which we crossed going southwards; the third sounding, at a depth of 2600 fathoms, is marked "red clay."

According to our present experience, the deposit of "*Globigerina*-ooze" is limited to water of a certain depth, the extreme limit of the pure characteristic formation being placed at a depth of somewhere about 2250 fathoms. Crossing from these shallower regions occupied by the ooze into deeper soundings, we find universally that the calcareous formation gradually passes into, and is finally replaced by, an extremely fine pure clay, which occupies, speaking generally, all depths below 2500 fathoms, and consists, almost entirely, of a silicate of the red oxide of iron and alumina. The transition is very slow, and extends over several hundred fathoms of increasing depth; the shells gradually lose their sharpness of outline and assume a kind of "rotten" look and a brownish colour, and become more and more mixed with a fine amorphous red-brown powder, which increases steadily in proportion until the lime has almost entirely disappeared. This brown matter is in the finest possible state of subdivision, so fine that when, after sifting it to separate any organisms it might contain, we put it into jars to settle, it remained for days in suspension, giving the water very much the appearance and colour of chocolate.

In indicating the nature of the bottom on the charts, we came from experience, and without any theoretical consideration, to use three terms for soundings in deep water. Two of these, Gl. oz. and r. cl., were very definite, and indicated strongly marked formations, with apparently but few characters in common; but we frequently got soundings which we could not exactly call either "*Globigerina*-ooze" or "red clay;" and before we were fully aware of the nature of these we were in the habit of indicating them as "grey ooze" (gr. oz.). We now recognize the "grey ooze" as an intermediate stage between the *Globigerina*-ooze and the red clay; we find that, on one side as it were of an ideal line, the red clay contains more and more of the material of the calcareous ooze, while, on the other, the ooze is mixed with an increasing proportion of "red clay."

Although we have met with the same phenomenon so frequently, that we were at length able to predict the nature of the bottom from the

depth of the sound, with absolute certainty, for the Atlantic and the Southern Sea, we had perhaps the best opportunity of observing it in our first section across the Atlantic, between Teneriffe and St. Thomas. The first four stations on this section (Pl. IV.), at depths from 1525 to 2220 fathoms, show "*Globigerina*-ooze." From the last of these, which is about 300 miles from Teneriffe, the depth gradually increases to 2740 fathoms, at 500, and 2950 fathoms, at 750 miles from Teneriffe. The bottom in these two soundings might have been called "grey ooze;" for although its nature has altered entirely from the "*Globigerina*-ooze," the red clay into which it is rapidly passing still contains a considerable admixture of carbonate of lime.

The depth goes on increasing, to a distance of 1150 miles from Teneriffe, when it reaches 3150 fathoms; there the clay is pure and smooth, and contains scarcely a trace of lime. From this great depth the bottom gradually rises, and, with decreasing depth, the grey colour and the calcareous composition of the ooze return. Three soundings in 2050, 1900, and 1950 fathoms on the "Dolphin Rise," gave highly characteristic examples of the *Globigerina* formation. Passing from the middle plateau of the Atlantic into the western trough, with depths a little over 3000 fathoms, the red clay returns in all its purity: and our last sounding, in 1420 fathoms, before reaching Sombrero, restored the *Globigerina*-ooze with its peculiar associated fauna.

This section shows also the wide extension and the vast geological importance of the red-clay formation. The total distance from Teneriffe to Sombrero is about 2700 miles. Proceeding from east to west, we have

About	80	miles	of	volcanic mud and sand.
„	350	„	„	"*Globigerina*-ooze,"
„	1050	„	„	"red clay,"
„	330	„	„	"*Globigerina*-ooze,"
„	850	„	„	"red clay,"
„	40	„	„	"*Globigerina*-ooze,"

giving a total of 1900 miles of red clay to 720 miles of *Globigerina*-ooze.

The following Table, taken from the chart, gives a good general idea of the distribution of the two formations with respect to depth. It cannot of course be taken as exact; the indications were jotted down from the impression of colour given at the time, and there is no hard and fast line between *Globigerina*-ooze and grey ooze on the one hand, and between red clay and grey ooze on the other. The Table gives an average depth of 1800 fathoms for our soundings in the *Globigerina*-ooze. This is a datum of no value; for we only rarely sounded in shallow water, and we know that this formation covers large areas at depths between 300 and 400 fathoms; but the mean maximum depth at which it occurs is important, and that may be taken from the Table as about 2250 fathoms. The mean depth at which we find the transition grey ooze is 2400 fathoms; and the mean depth of the red-clay soundings is

about 2700 fathoms. The general concurrence of so many observations would go far to prove, what seems now to stand, indeed, in the position of an ascertained fact, that wherever the depth increases from about 2200 to 2600 fathoms, the modern Chalk formation of the Atlantic and of other oceans passes into a clay.

No. of Station.	Nature of the Bottom.		
	Glob. Ooze.	Grey Ooze.	Red Clay.
From Cape Finisterre to Teneriffe.			
I.	1125		
	1975		
II.	470		
	1800		
III.	1000		
VI.	1525		
From Teneriffe to St. Thomas.			
1.	1890		
2.	1945		
4.	2220		
5.	..	..	2740
6.	..	..	2950
7.	..	..	2750
8.	..	..	2800
9.	..	..	3150
10.	..	..	2720
11.	..	..	2575
12.	2025		
13.	1900		
14.	1950		
15.	..	..	2325
16.	..	..	2435
17.	..	..	2385
18.	..	..	2675
19.	..	..	3000
20.	..	..	2975
21.	..	..	3025
22.	1420		
23.	450		
From St. Thomas to Bermudas.			
25.	..	3875	
26.	..	2800	
27.	..	2960	
28.	..	..	2850
29.	..	..	2700
30.	..	..	2600
31.	..	2475	
32.	..	2250	
..	..	1820	

TABLE (*continued*).

No. of Station.	Nature of the Bottom.		
	Glob. Ooze.	Grey Ooze.	Red Clay.
	From Bermudas to Halifax.		
37.	..	2650	
38.	..	2600	
39.	..	2850	
42.	..	2425	
44.	..	1700	
	From Halifax to Bermudas.		
50.	..	1250	
51.	..	2200	
52.	..	2800	
53.	..	2650	
54.	..	2650	
55.	..	2500	
	From Bermudas to the Azores.		
58.	..	1500	
59.	..	2360	
60.	..	2575	
61.	..	2850	
62.	..	2875	
63.	..	2750	
65.	..	2700	
66.	..	2750	
67.	..	2700	
68.	..	2175	
69.	..	2200	
70.	1675		
71.	1675		
72.	1240		
73.	1000		
74.	1350		
76.	900		
	From the Azores to Madeira.		
78.	1000		
79.	2025		
80.	2660		
81.	2675		
82.	2400		
83.	1650		
	From Madeira to Cape Verde Islands.		
86.	2300		
88.	2300		
89.	2400		
90.	2400		
91.	2075		
92.	1975		

TABLE (*continued*).

No. of Station.	Nature of the Bottom.		
	Glob. Ooze.	Grey Ooze.	Red Clay.
From the Cape Verde Islands to St. Paul Rocks.			
95.	2300		
97.	2575		
98.	1750		
102.	..	2450	
104.	..	2500	
105.	..	2275	
106.	1850		
107.	1500		
108.	1900		
From the St. Paul Rocks to S. Salvador.			
110.	2275		
111.	2475		
112.	2200		
5.	2150		
6.	2275		
From S. Salvador to Tristan d'Acunha.			
129.	..	..	2150
130.	..	..	2350
131.	2275		
132.	2050		
133.	1900		
134.	2025		
From Tristan d'Acunha to the Cape of Good Hope.			
137.	..	..	2550
138.	..	..	2650
139.	..	2325	
140.	..	1250	
From the Cape of Good Hope to Kerguelen Island.			
143.	1900		
144.	1570		
146.	1375		
147.	1600		
From Kerguelen Island to Melbourne.			
158.	1800		
159.	2150		
160.	..	..	2600

The nature and origin of this vast deposit of clay is a question of the very greatest interest; and although I think there can be no doubt that it is in the main solved, yet some matters of detail are still involved in

difficulty. My first impression was, that it might be the most minutely divided material, the ultimate sediment, produced by the disintegration of the land, by rivers and by the action of the sea on exposed coasts, and held in suspension and distributed by ocean currents, and only making itself manifest in places unoccupied by the *Globigerina*-ooze. Several circumstances seemed, however, to negative this mode of origin. The formation seemed too uniform; whenever we met with it, it had the same character, and it only varied in composition in containing less or more carbonate of lime.

Again, we were gradually becoming more and more convinced that all the important elements of the *Globigerina*-ooze lived on the surface; and it seemed evident that, so long as the conditions on the surface remained the same, no alteration of contour at the bottom could possibly prevent its accumulation; and the surface conditions in the Mid-Atlantic were very uniform, a moderate current of a very equal temperature passing continuously over elevations and depressions, and everywhere yielding to the tow-net the ooze-forming Foraminifera in the same proportion. The Mid-Atlantic swarms with pelagic Mollusca; and, in moderate depths, the shells of these are constantly mixed with the *Globigerina*-ooze, sometimes in number sufficient to make up a considerable portion of its bulk. It is clear that these shells must fall in equal numbers upon the red clay; but scarcely a trace of one of them is ever brought up by the dredge on the red-clay area. It might be possible to explain the absence of shell-secreting animals living on the bottom by the supposition that the nature of the deposit was injurious to them; but then the idea of a current sufficiently strong to sweep them away is negatived by the extreme fineness of the sediment which is being laid down; the absence of surface shells appears to be intelligible only on the supposition that they are in some way removed.

We conclude, therefore, that the "red clay" is not an additional substance introduced from without, and occupying certain depressed regions on account of some law regulating its deposition; but that it is produced by the removal, by some means or other, over these areas, of the carbonate of lime which forms probably about 98 per cent. of the material of the *Globigerina*-ooze. We can trace, indeed, every successive stage in the removal of the carbonate of lime in descending the slope of the ridge or plateau where the *Globigerina*-ooze is forming, to the region of the clay. We find, first, that the shells of pteropods and other surface Mollusca, which are constantly falling on the bottom, are absent, or if a few remain they are brittle and yellow, and evidently decaying rapidly. These shells of Mollusca decompose more easily, and disappear sooner, than the smaller and apparently more delicate shells of Rhizopods. The smaller Foraminifera now give way and are found in lessening proportion to the larger; the coccoliths first lose their thin outer border and then disappear, and the clubs of the rhabdoliths get worn out of shape and are last seen,

under a high power, as minute cylinders scattered over the field. The larger Foraminifera are attacked, and instead of being vividly white and delicately sculptured, they become brown and worn, and finally they break up, each according to its fashion; the chamber-walls of *Globigerina* fall into wedge-shaped pieces which quickly disappear, and a thick rough crust breaks away from the surface of *Orbulina*, leaving a thin inner sphere, at first beautifully transparent, but soon becoming opaque and crumbling away.

In the mean time, the proportion of the amorphous "red clay" to the calcareous elements of all kinds, increases, until the latter disappear, with the exception of a few scattered shells of the larger Foraminifera, which are still found, even in the most characteristic samples of the "red clay."

There seems to be no room left for doubt that the red clay is essentially the insoluble residue, the *ash*, as it were, of the calcareous organisms which form the *Globigerina*-ooze, after the calcareous matter has been by some means removed. An ordinary mixture of calcareous Foraminifera with the shells of Pteropods, forming a fair sample of "*Globigerina*-ooze" from near St. Thomas, was carefully washed and subjected by Mr. Buchanan to the action of weak acid; and he found that there remained, after the carbonate of lime had been removed, about one per cent. of a reddish mud, consisting of silica, alumina, and the red oxide of iron. This experiment has been frequently repeated with different samples of "*Globigerina*-ooze," and always with the result that a small proportion of a red sediment remains, which possesses all the characters of the "red clay."

In the *Globigerina*-ooze, siliceous bodies, including the spicules of Sponges, the spicules and tests of Radiolarians, and the frustules of Diatoms occur in appreciable proportion; and these also diminish in number, and the more delicate of them disappear in the transition from the calcareous ooze to the "red clay."

I have already alluded to the large quantity of nodules of the peroxide of manganese which were brought up by the trawl from the red-clay area on the 13th of March. Such nodules seem to occur universally in this formation. No manganese can be detected in the *Globigerina*-ooze; but no sooner has the removal of the carbonate of lime commenced than small black grains make their appearance, usually rounded and mammillated on the surface, miniatures, in fact, of the larger nodules which abound in the clay; and, at the same time, any large organic body, such as a shark's tooth, that may happen to be in the ooze is more or less completely replaced by manganese; and any inorganic body, such as a pebble or a piece of pumice, is coated with it, as a fine black mammillated layer. It is not easy to tell what the proportion of manganese in the red clay may be, but it is very considerable. At station 160, on the 13th of March, the trawl brought up nearly a bushel of nodules, from the size of a walnut to that of an orange; but these were probably the result of the sifting of a large quantity of the

clay. The manganese is doubtless, like the iron, set free by the decomposition of the organic bodies and tests. It is known to exist in the ash of some Algæ to the amount of 4 per cent.

The interesting question now arises as to the cause and method of the removal of the carbonate of lime from the cretaceous deposit; and on this matter we are not yet in a position to form any definite conclusion.

One possible explanation is sufficiently obvious. All sea-water contains a certain proportion of free carbonic acid, and Mr. Buchanan believes that he finds it rather in excess in bottom-water from great depths. At all events, the quantity present is sufficient to convert into a soluble compound, and thus remove a considerable amount of carbonate of lime. If the balance of supply be very delicately adjusted, it is just conceivable that the lime in the shells, in its fine state of subdivision, having been attacked by the sea-water from the moment of the death of the animal, may be entirely dissolved during its retarded passage through the half mile or so of water of increasing density. A great deal of the bottom-water in these deep troughs has been last at the surface, in the form of circumpolar freshwater ice; and though fully charged with carbonic acid, it is possible that it may be comparatively free from carbonate of lime, and that its solvent power may thus be greater.

The red clay or, more probably, the circumstances which lead to its deposition seem on the whole unfavourable to the development of animal life. Where its special characters are most marked, no animals which require much carbonate of lime for the development of their tissues or of their habitations appear to exist. Our growing experience is, that although animal life is possible at all depths, after a certain depth, say 1500 fathoms, its abundance diminishes. This would seem to indicate that the extreme conditions of vast depths are not favourable to its development; and one might well imagine that the number of shell-building animals might decrease, until the supply of lime was so far reduced as to make it difficult for them to hold their own against the solvent power of the water of the sea—just as in many districts where there is little lime, the shells of land and freshwater mollusks are light and thin, and the animals themselves are stunted and scarce.

It seems, however, that neither the extreme depth at which the red clay is found, nor the conditions under which it is separated and laid down, are sufficient entirely to negative the existence of living animals, even of the higher invertebrate orders. In several of the hauls, we brought up Holothurids of considerable size, with the calcareous neck-rings very rudimentary, and either no calcareous bodies in the test, or a mere trace of such. Nearly every haul gave us delicate branching Bryozoa, with the zooëcium almost membranous. One fortunate cast, about 150 miles from Sombrero, brought up, from a depth of 2975 fathoms, very well-marked red mud, which did not effervesce with hydrochloric acid. Entangled in the dredge, and imbedded in the mud, were many of

the tubes of a tube-building annelid, several of them 3 to 4 inches long, and containing the worm, a species of *Myriochele*, still living. The worm-tubes, like all the tests of Foraminifera from the same dredging, were made up of particles of the red clay alone.

It seems evident, from the observations here recorded, that *clay*, which we have hitherto looked upon as essentially the product of the disintegration of older rocks, under certain circumstances, may be an organic formation like chalk; that, as a matter of fact, an area on the surface of the globe, which we have shown to be of vast extent, although we are still far from having ascertained its limits, is being covered by such a deposit at the present day.

It is impossible to avoid associating such a formation with the fine, smooth, homogeneous clays and schists, poor in fossils, but showing worm-tubes and tracks, and bunches of doubtful branching things, such as *Oldhamia*, siliceous sponges, and thin-shelled peculiar shrimps. Such formations, more or less metamorphosed, are very familiar, especially to the student of palæozoic geology, and they often attain a vast thickness. One is inclined, from this great resemblance between them in composition and in the general character of the included fauna, to suspect that these may be organic formations, like the modern red clay of the Atlantic and Southern Sea, accumulations of the insoluble ashes of shelled creatures.

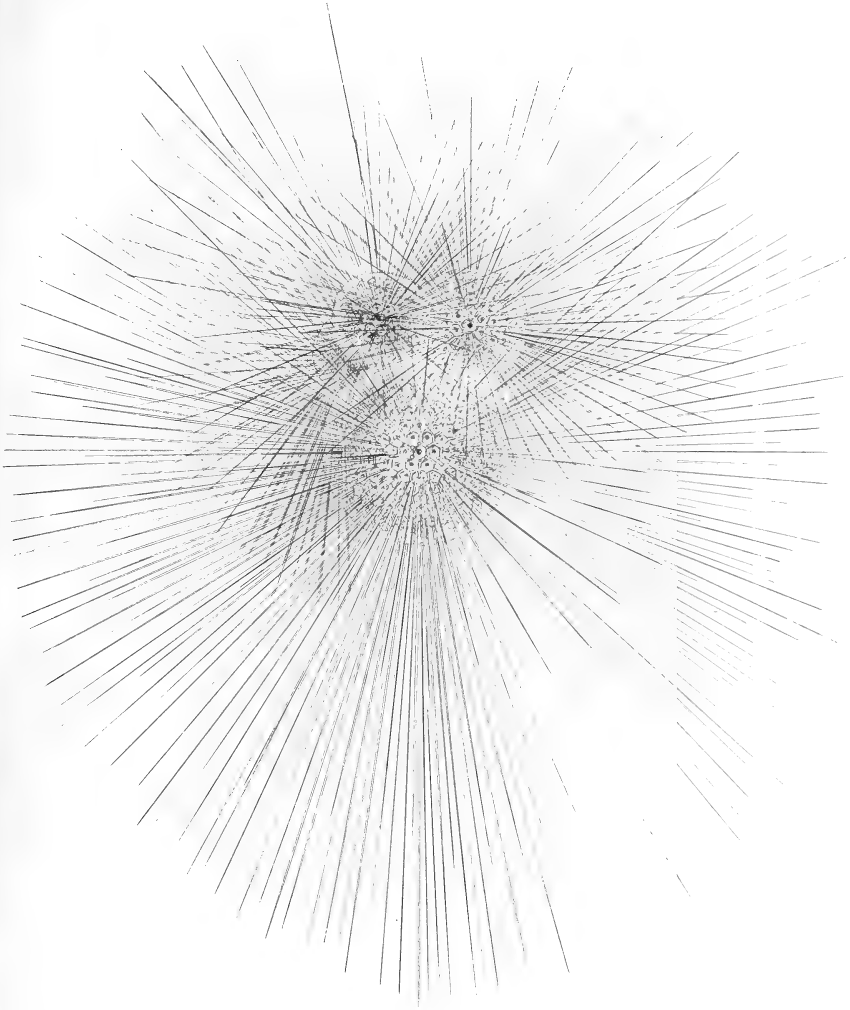
The dredging in the red clay on the 13th of March was unusually rich. The bag contained examples, those with calcareous shells rather stunted, of most of the characteristic deep-water groups of the Southern Sea, including *Umbellularia*, *Euplectella*, *Pterocrinus*, *Brisinga*, *Ophioglypha*, *Pourtalesia*, and one or two Mollusca. This is, however, very rarely the case. Generally the "red clay" is barren, or contains only a very small number of forms.

On the 11th of February, lat. 60° 52′ S., long. 80° 20′ E., and March 3, lat. 53° 55′ S., long. 108° 35′ E., the sounding-instrument came up filled with a very fine cream-coloured paste, which scarcely effervesced with acid, and dried into a very light impalpable white powder. This, when examined under the microscope, was found to consist almost entirely of the frustules of Diatoms, some of them wonderfully perfect in all the details of their ornament, and many of them broken up. The species of diatoms entering into this deposit have not yet been worked up, but they appear to be referable chiefly to the genera *Fragillaria*, *Coscinodiscus*, *Chætoceros*, *Asteromphalus*, and *Dictyocha*, with fragments of the separated rods of a singular siliceous organism, with which we were unacquainted, and which made up a large proportion of the finer matter of this deposit. Mixed with the Diatoms there were a few small *Globigerinæ*, some of the tests and spicules of Radiolarians, and some sand particles; but these foreign bodies were in too small proportion to affect the formation as consisting practically of diatoms alone. On the 4th of February, in lat. 52° 29′ S., long. 71° 36′ E., a little to the north of the

Heard Islands, the tow net, dragging a few fathoms below the surface, came up nearly filled with a pale yellow gelatinous mass. This was found to consist entirely of Diatoms of the same species as that found at the bottom. By far the most abundant was the little bundle of siliceous rods (Pl. III. fig. 5) fastened together loosely at one end, separating from one another at the other end, and the whole bundle loosely twisted into a spindle. The rods are hollow, and contain the characteristic endochrome of the *Diatomaceæ*. Like the "*Globigerina*-ooze," then, which it succeeds to the southward in a band apparently of no great width, the materials of this siliceous deposit are derived entirely from the surface and intermediate depths. It is somewhat singular that Diatoms did not appear to be in such large numbers on the surface over the Diatom-ooze as they were a little further north. This may perhaps be accounted for by our not having struck their belt of depth with the tow-net; or it is possible that when we found it, on the 11th of February, the bottom deposit was really shifted a little to the south by the warm current, the excessively fine flocculent *débris* of the Diatoms taking a certain time to sink. The belt of Diatom-ooze is certainly a little further to the southward in long. 80° E. in the path of the reflux of the Agulhas current, than in long. 108° E.

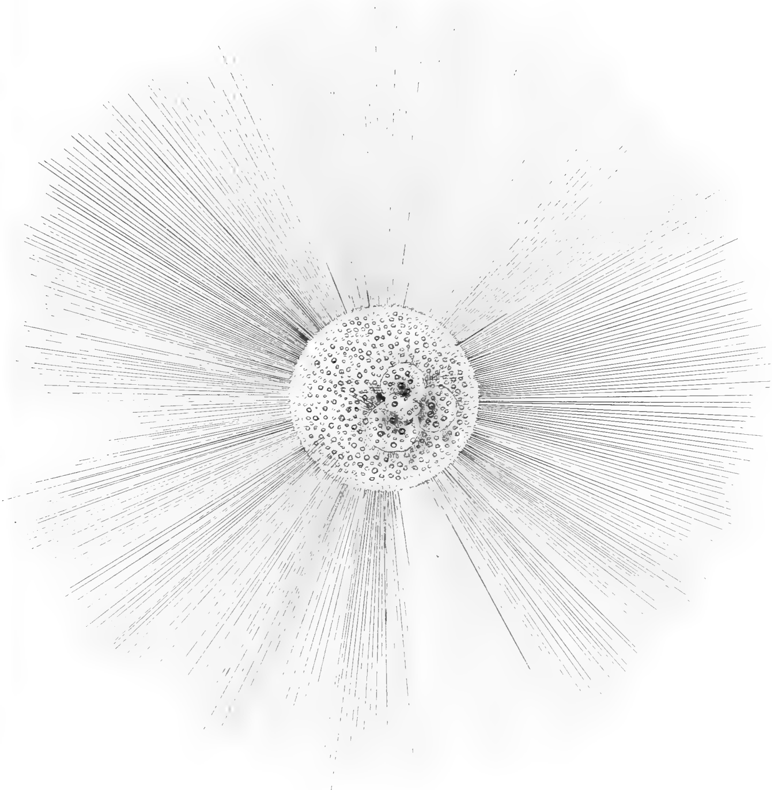
All along the edge of the ice pack—everywhere in fact to the south of the two stations, that of the 11th of February, on our southward voyage, and that of the 3rd of March, on our return, we brought up fine sand and greyish mud, with small pebbles of quartz and felspar, and small fragments of mica-slate, chlorite-slate, clay-slate, gneiss, and granite. This deposit, I have no doubt, was derived from the surface like the others, but, in this case, by the melting of icebergs and the precipitation of foreign matter contained in the ice.

We never saw any trace of gravel or sand, or any material necessarily derived from land, on an iceberg. Several showed vertical or irregular fissures filled with discoloured ice or snow; but, when looked at closely, the discoloration proved usually to be very slight, and the effect, at a distance, was usually due to the foreign material which filled the fissure reflecting light less perfectly than the general surface of the berg. I conceive that the upper part of one of these great tabular southern icebergs, including by far the greater part of its bulk, and culminating in the portion exposed above the surface of the sea, was formed by the piling up of successive layers of snow throughout the period, amounting perhaps to several centuries, during which the ice-cap was slowly forcing itself over the low land and out to sea, over a long extent of gentle slope, until it reached a depth considerably above 200 fathoms, when the lower specific weight of the ice caused an upward strain, which at length overcame the cohesion of the mass, and portions were rent off and floated away. If this be the true history of the formation of these icebergs, the absence of all land *débris* in the portion exposed above the surface of the



GLOBIGERINA.

W. H. Wesley sc.   W. West & Co. imp.



ORBULINA.

W.H. Wesley sc. W. West & Co. imp.

Fig. 2

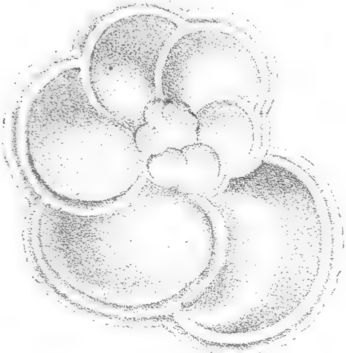


Fig. 1.

Fig. 3.

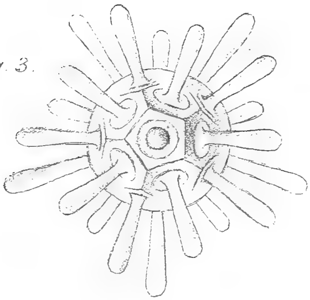


Fig. 4

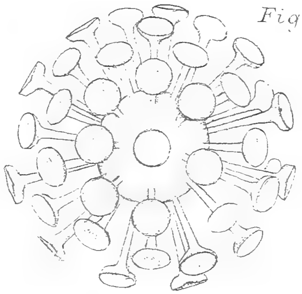
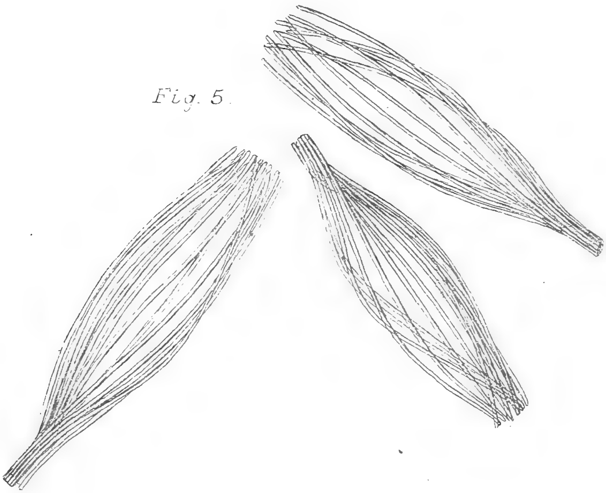


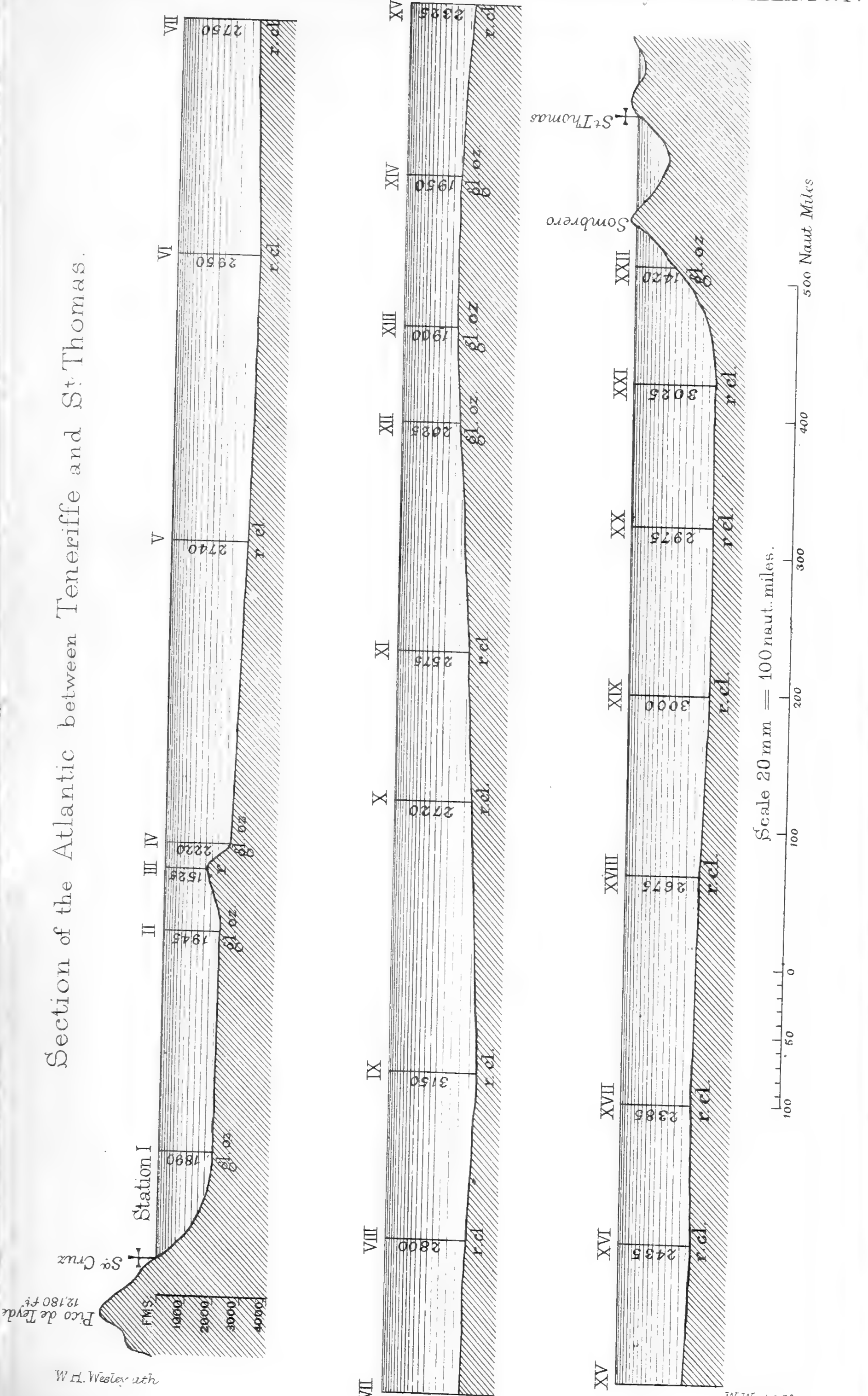
Fig. 5.



Figs 1 & 2. Pulvinulina Figs 3. & 4. Rhabdoliths, Fig. 5. Diatoms.

W.H. Wesley lith.

W. West & Co. imp.



W. H. Wesley lith.

W. West & Co. imp.

sea is readily understood. If any such exist it must be confined to the lower part of the berg, to that part which has, at one time or other, moved on the floor of the ice-cap.

The icebergs, when they are first dispersed, float in from 200 to 250 fathoms. When, therefore, they have been drifted to latitudes of 65° or 64° S., the bottom of the berg just reaches the layer at which the temperature of the water is distinctly rising; and it is rapidly melted, the mud and pebbles with which it is more or less charged being precipitated to the bottom. That this precipitation takes place all over the area where the icebergs are breaking up constantly, and to a considerable extent, is evident from the fact of the soundings being entirely composed of such deposits; for the Diatoms, *Globigerinæ*, and Radiolarians are present on the surface in large numbers; and unless the deposit from the ice were abundant it would soon be covered and masked by a layer of the exuvia of surface-organisms.

EXPLANATION OF THE PLATES.

Plates I. & II.

A *Globigerina* and an *Orbulina*, with the radiating processes entire.

Plate III.

Figs. 1 & 2. *Pulvinulina*. Figs. 3 & 4. Rhabdoliths. Fig. 5. The new Diatom.

November 30, 1874.

ANNIVERSARY MEETING.

WILLIAM SPOTTISWOODE, M.A., Treasurer and Vice-President, in the Chair.

General Boileau, for the Auditors of the Treasurer's Accounts on the part of the Society, reported that the total receipts during the past year, including a balance of £693 13*s.* 5*d.* carried from the preceding year, amount to £5726 3*s.* 3*d.*; and that the total expenditure in the same period amounts to £5451 11*s.* 5*d.*, leaving a balance at the Bankers of £236 18*s.* 8*d.*, and £37 13*s.* 2*d.* in the hands of the Treasurer.

The thanks of the Society were voted to the Treasurer and Auditors.

The Secretary read the following Lists :—

Fellows deceased since the last Anniversary.

On the Home List.

Neil Arnott, M.D.
Rev. James William Bellamy, B.D.
Sir William Fairbairn, Bart., LL.D.
Prof. Robert Edmond Grant, M.D.
Sir William Jardine, Bart.
Edwin Lankester, M.D.
David Livingstone, LL.D.
Sir James Ranald Martin, C.B.
Prof. John Phillips, LL.D.
Sir John Rennie, Knt.
The Right Hon. Sir George Rose, Knt.
Edward Smith, M.D.
Major-Gen. Sir John Mark Frederic Smith, R.E., K.H.
Edward Hamilton Stirling.

On the Foreign List.

Louis Agassiz.
Anders Jonas Ångström.
J. B. A. L. L. Elie de Beaumont.
Peter Andreas Hansen.
Philippe Gustave de Pontécoulant.
Lambert Adolphe Jacques Quetelet.

Withdrawn.

Prof. Charles Piazzi Smyth.

Change of Name and Title.

Alexander Robert Johnston *to* Campbell-Johnston.
Right Hon. Sir John Pakington, Bart., *to* Lord Hampton.

Fellows elected since the last Anniversary.

Edward Viscount Cardwell, F.G.S.
Isaac Lowthian Bell, F.C.S.
W. T. Blanford, F.G.S.
Henry Bowman Brady, F.L.S.
Thomas Lauder Brunton, M.D., Sc.D.
Prof. W. Kingdon Clifford, M.A.
Augustus Wollaston Franks, M.A.
Prof. Olaus Henrici, Ph.D.
Prescott G. Hewett, F.R.C.S.
John Eliot Howard, F.L.S.
Sir Henry Sumner Maine, LL.D.
Edmund James Mills, D.Sc.
Rev. Stephen Joseph Perry, F.R.A.S.
Henry Wyldbore Rumsey, M.D.
Alfred R. C. Selwyn, F.G.S.
Charles William Wilson, Major R.E.

The Treasurer (for the President) then addressed the Society as follows :—

Gentlemen,

It has been represented to me that, the Royal Society being now, after eighteen years of temporary accommodation, settled in quarters of which

we hope to retain undisturbed occupation for some generations to come, an account of the present position of the Society in respect of our more important possessions, foundations, and functions, and our relations to the Government, would not only be generally acceptable, but might even be required of me by that large and increasing class of Fellows who live far from our doors. This class now numbers as nearly as possible one half of the Society, few of whom can be even occasional attendants at our Meetings; and if to this class of absentees be added the large number of residents within the metropolitan district whose avocations prevent their attending, it will not surprise you to hear that (as I have ascertained by careful inquiry) a very large proportion of our fellow Members know little of the Society's proceedings beyond what appears in our periodical publications, nor of our collections, nor of the tenure under which we occupy our apartments under the Crown—and that many have never heard of the funds we administer, whether our own or those voted by Parliament in aid of scientific research, nor of the fund for relief of the necessitous, nor of the gratuitous services rendered by the Society to various departments of the Government.

Unlike the great Academies of the continent, the Royal Society has never published an Almanack or Annuaire containing information upon its privileges, duties, constitution, and management. Particulars on these points are for the most part now accessible to the Fellows only by direct inquiry, or through the Council Minutes; and these, to non-resident Fellows, are practically inaccessible. In my own case, though I have long been a resident Fellow and had the honour of serving on your Councils for not a few years, it was not until I was placed in the position I now hold that I became aware of the number and magnitude of the Society's duties, or of the responsibility these impose on your officers.

It is upwards of a quarter of a century since an account of the foundations that then existed and the work the Society then carried on was published in Weld's valuable, but too diffuse, 'History of the Royal Society.' These have all been greatly modified or extended since that period; and many others have been added to them; so that the time has now arrived when a statement of the large funds applicable to scientific research which the Society distributes, the conditions under which these are to be applied for, and other particulars, might with advantage be published in a summary form and distributed to the Fellows annually.

Finance.—After the financial statement made by the Auditors, you will, I am sure conclude that there is no cause for apprehension in respect of the Society's funds or income; and when to this I add that the expenses of removal from the old House, including new furniture, amount to £1300, and that the volume of Transactions for the present year will contain eighty-six Plates, the largest number hitherto executed at the

Society's cost within the same period, you will also conclude that there is no want of means for providing illustrations to papers communicated to us for publication.

The landed property of the Society, as stated in the printed balance-sheet now before you, consists of an estate at Acton, in the neighbourhood of London, and an estate at Mablethorpe, Lincolnshire; each yielding a good rental. The Acton estate, at present on lease to an agricultural tenant, is planned to be let as building land, for which it is favourably situate, and will thus become increasingly valuable.

The subject of the tenure under which the Society holds the apartments we now occupy was brought up on a question of Insurance. That question has been satisfactorily settled by reference to the Treasury; but it may still be worth while briefly to state the facts which the Council considered as furnishing valid grounds for appealing against the requirement to insure, and for at the same time requesting an assurance that the permanence of our tenure is in no way weakened by our removal to this building. These are:—that when the apartments in Somerset House were originally assigned to the Society by command of George III., they were granted "during the pleasure of the Crown without payment of rent or any other pecuniary consideration whatever;" that the Society was not required to insure either in Somerset House or old Burlington House; that when the Society removed at the request of the Government from Somerset House, and accepted temporary accommodation in Burlington House, it was under the written assurance of the Secretary of the Treasury, addressed to the President of the Society, that the claims of the Society to "permanent accommodation should not be thereby in any respect weakened;" that in the debate on the estimates in 1857, the Secretary of the Treasury stated, in his place in Parliament, that "the Society could not be turned out of Somerset House without its own consent," and that "it was entitled to rooms by Royal grant."

To this appeal the Lords Commissioners returned a satisfactory answer; and their letter, dated October 27th last, assures us "that there is no intention on the part of the Treasury to alter the terms on which the Royal Society holds its appointments under the Crown; the conditions of the Society's tenure will therefore be the same as those on which it occupied rooms in Somerset House, and was subsequently transferred to Burlington House."

While feeling it my duty to lay these details before you, I must accompany them with the assurance that nothing has occurred during this correspondence to disturb the unbroken harmony that has existed between Her Majesty's Government and the Royal Society ever since our occupation of apartments under favour of the Crown.

On every occasion of change of quarters the Society has received abundant proofs of the regard shown by the Government for its position,

requirements, and continued prosperity; and there is, I am sure, every disposition on the part of the Government to recognize the fact that the privileges conferred on the Society are fully reciprocated by the multifarious aid and advice furnished by your Council in matters of the greatest importance to the well-being of the State.

The practice of electing Fellows of the so-called privileged class whose qualifications were limited to accident of lineage or political status, has been viewed with grave dissatisfaction by many, ever since the election of ordinary Fellows was limited to fifteen. The Council has in consequence felt it to be its duty to give most careful attention to the subject, which it referred to a Committee, whose report has been adopted and embodied in a bye-law.

The privileged class consisted, as you are aware, of certain Royal personages, Peers of the Realm and Privy Councillors (Statutes, Sect. IV. cap. 1); and they were balloted for at any meeting of the Society, after a week's notice given on the part of any Fellow, without a suspended certificate, or other form whatever.

The Committee reported that it was desirable to retain the power of electing, as a "privileged class," persons who, while precluded by public duties or otherwise from meeting the scientific requirements customary in the case of ordinary Fellows, possessed the power and had shown the wish to forward the ends of the Society, and recommended that the class should be limited to the Princes of the Blood Royal, and members of Her Majesty's Privy Council. And with regard to the method of election, they recommended that a Prince of the Blood Royal might be publicly proposed at any ordinary meeting, and balloted for at the next; that, with regard to a member of Her Majesty's Privy Council, he might be proposed at any ordinary meeting by means of a certificate prepared in accordance with Chap. I. Sect. 3 of the Statutes, membership of the Privy Council being the only qualification stated—the certificate being, with the Society's permission, suspended in the meeting-room till the day of election, which should fall on the third ordinary meeting after suspension.

Having regard to the eminent services to the State which have been rendered by Privy Councillors, and to the fact that all Peers who do render such services are habitually enrolled on the list of Privy Councillors, it was believed by the Council that the effect of thus limiting the privileged class would be that the doors of the Society would remain open to all such Peers as desire and deserve admission, but who have not the ordinary qualifications for fellowship; while all such Peers as might appear with claims which compete with those of ordinary candidates would prefer owing the fellowship to their qualifications rather than to their birth.

The Council hopes that by this means the so-called privileged class

will be reinforced, and that statesmen who may have considered themselves inelegible through want of purely scientific qualifications, or who have hesitated to offer themselves from the fear of interfering with the scientific claims of others, will in future come forward and recruit our ranks.

A passing notice of the manner of proposing candidates for the ordinary class of fellowship may not be out of place. Theoretically this is done by a Fellow who is supposed to be a friend of the candidate, is versed in the science on which his claims are founded, and is satisfied of his fitness in all respects for fellowship. It is most desirable that the Fellow who proposes a candidate should take upon himself the whole duty and responsibility of preparing the certificate, should sign it first, and himself procure the signatures of other Fellows in whose judgment of the candidate's qualifications the Council and the Society may place implicit confidence. It is unsatisfactory to see attached to a candidate's certificate an ill-considered list of signatures, whether given from personal or from general knowledge; and the happily rare practice of soliciting signatures and support, directly or indirectly, by the candidate himself, cannot be too strongly deprecated. For obvious reasons the President, Officers, and other Members of Council have hitherto during their periods of office abstained from proposing a candidate of the ordinary class or from signing his certificate, but have not withdrawn their signatures from certificates sent in before they took office. The Council and Officers will probably not feel the same objection to signing the certificates of candidates of the privileged class, as these will not be selected for ballot by the Council, but will be elected by the Society at large at their ordinary meetings.

In carrying on the business of the Society the Council is much indebted to Committees appointed annually for special purposes, or to whom an occasional question is referred. The annual appointments include the Government-Grant, the Library, the Soirée, and the Acton-Estate Committees. The temporary Committees of the past year have been the Circumnavigation, the Transit-of-Venus-Expeditions, the Arctic, the House, the Brixham-Cave, the Privileged-Classes, and the Davy-Medal Committee. Besides these there are two permanent Committees, the Meteorological and the Scientific-Relief, to which fresh Members are appointed as vacancies occur. From these designations it will be understood that some of the Committees have been occupied with questions connected with the Government service, while others have devoted themselves exclusively to the business of the Society.

I shall now mention such of the labours of these Committees as seem to be most worthy of your attention.

The *Meteorological Committee of the Board of Trade*, as it ought to be called, discharges in all respects the most arduous and responsible duties of any, controlling as it does the whole machinery of the British

Government for the making, registering, and publishing of especially oceanic meteorological phenomena throughout the globe.

The primary purpose for which this and all similar offices were established, was the acceleration of ocean passages for vessels by an accurate investigation of the prevalent winds and currents. In other words, their great object is to aid the seaman in what Captain Basil Hall called "one of the chief points of his duty"—namely, "to know when to find a fair wind, and where to fall in with a favourable current." The first impulse to the formation of an Office for this purpose was given by the late General Sir J. Burgoyne, who in 1852 started the idea of land observations to be carried out by the Corps of Royal Engineers.

Shortly afterwards our Government corresponded with the United-States Government on the subject of cooperating in a scheme for land observations, which was followed by a suggestion on the part of America that the operations should be extended to the sea.

The correspondence was referred to the Royal Society, which warmly approved the scheme of sea observations, but saw many difficulties in carrying out that for the land. The Brussels Conference followed in 1853, when representatives of most of the maritime nations assembled and adopted a uniform plan of action. Soon after this, Lord Cardwell, then President of the Board of Trade, established the Meteorological Department of that office, and placed the late Admiral FitzRoy at the head of it—the Royal Society, at the request of the Government, supplying copious and complete instructions for his guidance, which were drawn up mainly by Sir Edward Sabine. Admiral FitzRoy's zeal and his great labours are known to all; he worked out the system of verifying and lending instruments, planning surveys, registering observations, publishing results; and, lastly, himself originated the plan of predicting the weather, and establishing storm-signals at the sea-ports along the coast.

On Admiral FitzRoy's death in 1865 the Royal Society was again consulted as to the position and prospects of the Office. Its Report, which did not differ materially from that of 1855, was in 1866 referred to a Committee, composed of a representative of the Board of Trade, of the Admiralty, and of the Royal Society. This Committee supported the previously expressed views of the Society, and suggested the placing of the Office under efficient scientific superintendence; upon which the Society, in the same year, was requested by the Government to undertake the superintendence of what had been the Meteorological Department of the Board of Trade. To this request the Council of the Society so far acceded as to nominate a Committee of eight Fellows (subsequently increased to ten) to undertake the entire and almost absolute control of the Office; and a Parliamentary grant of £10,000 per annum was provided to maintain it.

This is in brief the history of the connexion between the Royal Society and the Meteorological Office on the one hand, and between the Office and

the Government on the other. It is a very anomalous position, and has been greatly misunderstood. It has led to the misconception on the part of some that the Society controlled the Office, and by others that the Government (Board of Trade) controlled it, and by more that the annual grant of £10,000 is made to and in support of the Royal Society, or of its own objects, whereas the grant is paid direct to the Director of the Office as soon as voted. The Society's action is confined to the selection of the Committee, which superintends the Office, while the Board of Trade, leaving to the Committee the details of their operations, exercise only a general control. The labours of the Committee are entirely gratuitous, and no part of the £10,000 is touched by them or by the Royal Society.

I believe there is no parallel to such an organization as this in any other department of the Government. It has its advantage in securing to the Office absolute freedom from that disturbing element in the public offices, that their heads are chosen partly on political grounds and change with every Government, and its disadvantage in wanting the support of direct Government authority and prestige. Hitherto, owing to the care of the Committee, which meets almost weekly, to the zeal and efficiency of the Director (who is also Secretary to the Committee) and of the Marine Superintendent, it has worked well. Into its working it is not my purpose to enter; its efficiency and value are fully acknowledged by the public. No more practical proof of this can be cited than the general desire, supported by memorials presented to Parliament, for the restitution of the storm-signals, which were discontinued after Admiral FitzRoy's decease, on the ground of their trustworthiness having been called in question. It is no little testimony to the foresight of that zealous officer that they are not only now reestablished and in full working order at 100 stations on the coast of Great Britain, but that the very warnings issued from Paris to the coast of France by the Government of that country are actually sent to Paris from the Meteorological Office in London. The same warnings are transmitted along the whole European coast, from Norway to Spain; and the system has been extended to Italy, Portugal, and Australia.

The Kew Observatory, which is used also as the Central Observatory of the Meteorological Committee, is supported by a grant from that Committee, and by the munificence of our Fellow, Mr. Gassiot, who has settled on it a fund which produces about £500 a year for the carrying on of observations chiefly magnetical.

The Circumnavigation Committee.—The scientific results of the 'Challenger' Expedition have far exceeded our most sanguine anticipations. The Temperature Survey of the Atlantic may, as Dr. Carpenter informs me, be truly characterized as the most important single contribution ever made to Terrestrial Physics, presenting as it does the whole thermal

stratification of an oceanic area of about 15 million square miles and with an average depth of 15,000 feet. Nor are the results of the Pacific Survey less important. Some of these were laid before you at our meeting of the 26th inst. in Prof. Wyville Thomson's "Preliminary Notes on the Nature of the Sea-Bottom in the South Sea," which reveal the existence of hitherto unsuspected processes of aqueous metamorphism at great depths in the ocean, and throw an entirely new light upon the geological problem of the origin of "azoic" clays and schists.

Valuable papers on new and little-known marine animals have been contributed to our Transactions and Proceedings by Mr. Willemöes-Suhm, Mr. Moseley, and other members of the Civilian Scientific Staff of the 'Challenger;' and a Number of the Journal of the Linnean Society is devoted to the Botanical observations and collections made by Mr. Moseley during the course of the voyage.

Transit-of-Venus Committee.—Upon the representation of your Council, Her Majesty's Government has attached naturalists to two of the astronomical expeditions sent out from this country to observe the approaching transit of Venus. The stations selected were the two most inaccessible to ordinary cruisers, and at the same time most interesting in regard to their natural productions—namely, the island of Rodriguez in the Mauritius group, and Kerguelen's Land in the South Indian Ocean.

The objects and importance of these appointments were laid before the Government in the following statement:—

"It is an unexplained fact in the physical history of our globe, that all known oceanic archipelagos distant from the great continents, with the sole exceptions of the Seychelles and of a solitary islet of the Mascarene group (which islet is Rodriguez), are of volcanic origin. According to the meagre accounts hitherto published, Rodriguez consists of granite overlaid with limestone and other recent rocks, in the caves of which have been found the remains of recently extinct birds of a very singular structure. These facts, taken together with what is known of the Natural History of the volcanic islets of Mauritius and Bourbon to the west of Rodriguez and of the granitic archipelago of the Seychelles to the north of it, render an investigation of its natural products a matter of exceptional scientific interest, which, if properly carried out, cannot fail to be productive of most important results.

"As regards Kerguelen's Land, this large island (100 by 50 miles) was last visited in 1840, by the Antarctic Expedition under Sir James Ross, in midwinter only, when it was found to contain a scanty flora of flowering plants, some of which belong to entirely new types, and an extraordinary profusion of marine animals and plants of the greatest interest, many of them being representatives of north-temperate and Arctic forms of life.

"H.M.S. 'Challenger' will no doubt visit Kerguelen's Land, and

collect largely; but it is evident that many years would be required to obtain even a fair representation of its marine products; and though we are not prepared to say that the scientific objects to be obtained by a naturalist's visit to Kerguelen's Land are of equal importance to those which Rodriguez will yield, we cannot but regard it as in every respect most desirable that the rare opportunity of sending a collector to Kerguelen's Land should not be lost."

I may further state as a matter of great scientific interest, that Rodriguez contains the remains of a gigantic species of land-tortoise allied to those still surviving in some other islands of the Mauritian group, and that the nearest allies of these are the gigantic tortoises of the Galapagos Islands in the opposite hemisphere of the globe, as one of our Fellows, Dr. Günther, has shown in a paper read last Session to the Society. Very valuable collections of these fossils have been made by Mr. Newton, the Colonial Secretary of Mauritius, during a brief stay which he was enabled to make in Rodriguez; but the materials are far from sufficient for obtaining all the information we want.

In accordance with your Council's recommendation, the Treasury sanctioned the appointment of four naturalists—three to Rodriguez, and one to Kerguelen's Land. Those sent out to Rodriguez are:—Mr. I. B. Balfour, son of Prof. Balfour, of Edinburgh, F.R.S., who, besides being educated as a botanist, has worked as a field geologist in the Geological Survey of Scotland; he is charged with the duties of botanist and geologist; Mr. George Gulliver, son of one of our Fellows and a pupil of Professor Rolleston, in Oxford, who goes out as naturalist; and Mr. H. H. Slater, who has had great experience as a cave-explorer, and who will devote his attention especially to the collection of fossils.

The Kerguelen's-Land duties are undertaken by the Rev. A. E. Eaton, M.A., a gentleman most favourably known as an entomologist, and who had made very important collections in Spitzbergen, which he visited for the purpose of studying its fauna and flora. These gentlemen had, by the last accounts, all proceeded to their destinations.

Committee of Papers.—The strength of the Society being represented by its publications, the Committee of Papers is the one whose functions are unquestionably the highest and most onerous, as they are the most closely scrutinized by the Fellows and the public.

Every member of the Council is included in this Committee, which meets after almost every Council-meeting; and no part of its duties is at present performed by a subcommittee. It appears to me to be very doubtful whether this arrangement, even if the best, can last, owing to the greatly increased number of papers now communicated and their augmenting bulk, and to the value of their contents being less easily estimated as the subjects of scientific research become more specialized. As it is, in the majority of cases but few of the members present can

judge of the merits of many of the papers; and it is not easy after a protracted Council-meeting, and one occupied with promiscuous business, to fix the attention of a large Committee upon subjects with which but few members present may be familiar. It is true that the Committee is aided in all cases by the written opinions of careful and impartial referees, and by the special attainments of our Secretaries, and that it is most desirable that the sometimes divergent opinions of these should be weighed by others as well as by experts in the subjects of the papers. But for all this a Committee of the whole Council is not necessary; and though I should not be disposed to advocate a return to a system once pursued of resolving the Committee into subcommittees charged with special subjects, I think it possible that some other plan may meet the difficulties of the case and relieve our overburthened Council of much labour. A possible plan for relieving both the Council and the Committee, while securing as careful a scrutiny of the Papers as we now have, would be a division of the labours of the Committee, and an addition of extra members to its number, chosen from among the Fellows, who should continue in office throughout the Session. This, or some plan of the kind, would have the advantage of engaging more of the Fellows than at present in the affairs of the Society; and I feel sure that so responsible a position as that of Extra Member of the Committee of Papers would be accepted with pride by those Fellows who are most competent to discharge the duties.

It seems convenient to refer here to suggestions that have been made to me as to the expediency of breaking up our Transactions or Proceedings, or both, into sections devoted to Physics and Biology respectively, or even subdividing them still more. This separation has been advocated on the ground that science has become so specialized that no scientific man can grasp all its subdivisions, that the mixed publications are cumbersome and difficult to consult, and that private libraries are now overburthened with the publications of Societies, of each of which a small part would suffice for all their possessors' wants. There is no question that this, if now an evil, will soon become intolerable; for our publications increase rapidly in number of contributions, and in their bulk. There are, however, so many considerations to be discussed before any system of relief can be adopted, that I confine myself to stating the subject as it has been urged upon me.

The Society's Library now comprehends 36,270 volumes and 10,000 tracts, the most considerable collection of scientific works in the possession of any private body; and in respect of Transactions and Proceedings of Scientific Academies, Societies, and Institutions, I believe it is unrivalled among public bodies.

A complete Catalogue of the Scientific Books, MSS., and Letters, which I regret to say is unaccompanied by any historical or other information regarding the Library, was printed in 1839. Another Catalogue

of the Miscellaneous Literature and Letters was printed in 1841; and there is also a manuscript Catalogue of Maps, Charts, Engravings and Drawings, numbering upwards of 5000.

For some years past the Library Committee, indefatigable in steady endeavour, have greatly increased the value and efficiency of our Library; and in 1873, previous to leaving old Burlington House for our present apartments, it ordered a rearrangement of the whole, and the preparation of a new Catalogue, which is being proceeded with as fast as the current duties of the officers will permit.

In the mean time the Catalogue of Transactions and Journals is printed for working-purposes, and will be added to until such time as the general Catalogue is ready for press.

The collection of Oriental MSS. presented by Sir William Jones in 1792, and added to by his widow in 1797, was largely consulted by several of the distinguished foreigners who assembled at the Oriental Congress in London last September. From conversation with some of these gentlemen, I learnt that the collection contains many documents of the greatest value and rarity, together with some that are unique; and it may be worth the consideration of the Council, whether they would not be more useful if transferred to, or deposited in, the India Office or some other Oriental Library, where they would be consulted to greater advantage than here? At present they occupy part of the room devoted to our Archives.

The two most noteworthy additions to the Library during the past year have been:—the MSS. on logic and mathematics of our late fellow Professor Boole, presented by his widow; and Dr. Fayrer's collection of 47 original drawings of the poisonous snakes of India, which are of interest in connexion with his and Dr. Brunton's experiments on snake-poisons, printed in our 'Proceedings.'

The apartments devoted to the Library afford space for 20 years' addition at the present rate of increase; they are remarkably commodious; and those who assembled at our Soirée last spring and saw them for the first time lighted up and decorated, will consider with me that they are not only a noble suite of apartments, but that they are in keeping with the purposes and the high position of the Society.

You are aware that the Council resolved that the Catalogue of Scientific Papers should be continued through the decade 1864–1873. This work is now progressing under direction of the Library Committee, who have had charge of the undertaking from the commencement. The necessary funds are granted by a vote of the Council; and we may hope, in the course of the coming year, that the seventh volume of this important work will be ready for publication. And we confidently trust that the Government will extend its liberality by printing this as it did the former volumes of the series. The total outlay upon the six volumes already published (which comprise papers published between 1800 and 1863)

has been £8936 12*s*., of which £3720 15*s*. 6*d*. (the cost of preparation) was defrayed by the Society, and the rest (the cost of printing, paper, and binding) by the Treasury; against which must be set the proceeds of sale, repaid to the Treasury in occasional amounts, the last within the present year, making a total of one thousand pounds.

The number of copies of the Society's Transactions distributed gratuitously to Institutions and Individuals not Fellows of the Society is now 209, and of the Proceedings 325.

House Committee.—The great labours of this Committee in connexion with the removal into the apartments we now occupy had not terminated at the beginning of the past Session; and various matters have still to be attended to. That the arrangements the Committee has made have given satisfaction to the Fellows at large has been amply acknowledged. We are, indeed, greatly indebted to them for the knowledge, experience, and time all so freely given in our service, as also to the knowledge of our requirements and the practical views of our Assistant-Secretary, upon whom fell the duty of suggesting the best disposition of the apartments throughout this large and commodious building. Lastly, I would beg your permission to record the services of the eminent architect, Mr. Barry, who has throughout shown the greatest regard to our position and requirements, and but for whose professional ability enlisted in our service we might have found ourselves as ill as we are now well accommodated.

Funds and Bequests.—The Donation Fund. In 1828 our former President, Dr. Wollaston, invested £2000 in the Three per Cents for the creation of a Fund, the dividends from which were to be expended liberally "from time to time in promoting experimental researches, or in rewarding those by whom such researches have been made, or in such other manner as shall appear to the President and Council for the time being most conducive to the interests of the Society in particular, or of Science in general." There is no restriction as regards nationality; but Members of Council are excluded from participation during their term of office.

To this Fund many liberal additions were made: Mr. Davies Gilbert gave £1000; Warburton, Hatchett, Guillemard, and Chantrey each contributed 100 guineas. From these gifts, and by accumulations, the Fund in 1849 had increased to £5293. With subsequent contributions, and a bequest of £500 by our eminent Fellow the late Sir Francis Ronalds, the total, as shown by the balance-sheet now in your hands, amounts to £5816 1*s*. 1*d*. In addition to the balance-sheet already referred to, a detailed statement of grants from the Donation Fund is, in accordance with a resolution of Council, published with the Report of the Anniversary Meeting.

Sir Francis Ronalds died in 1873; his bequest (reduced by payment of legacy duty to £450) was made, as declared in his Will, in recognition

of the advantages he had derived when Honorary Director of the Observatory at Kew, from the sums granted to him out of the Fund to aid him in the construction of his photographic apparatus for the registration of Terrestrial Magnetism, Atmospheric Electricity, and other Meteorological phenomena.

Of the grants made during the past Session, I would especially mention £100 to Dr. Dohrn in support of the Stazione Zoologica at Naples, in which two British naturalists, Mr. Lankester and Mr. Balfour, have recently made a valuable series of observations on marine animals.

Among the others were a grant of £25 to Dr. Carpenter for the purpose of constructing an apparatus to illustrate the theory of Oceanic Circulation in relation to temperature, and £50 in aid of the Sub-Wealden Exploration. In reference to this last, I should remark that, in recognition of the important scientific results which have been obtained from the Sub-Wealden boring (which is now carried to a depth of 1000 feet), and in view of obtaining further assistance from Her Majesty's Government towards the work, the Council authorized me to lay before the Chancellor of the Exchequer such a statement as I should judge appropriate with the object of obtaining a grant from the public purse in aid of the boring.

In pursuance of this resolution, I joined the Presidents of the Geological Society and of the Institution of Civil Engineers in presenting a Memorial, which was most favourably received, and was answered by a promise on the part of the Treasury of £100 for every 100 feet of boring that should be accomplished, down to a depth of 2000 feet.

The Government Grant (of £1000 per annum) continues to be expended with satisfactory results. I must refer you to the report which will be published in our Proceedings for the statement of the grants, making, however, special allusion to Dr. Klein's work on the Anatomy of the Lymphatic system, towards which £100 from this fund was granted, and by means of which copies have been distributed to the best advantage in this country and abroad.

The Scientific-Relief Fund slowly augments, and has been of the greatest service. It is almost unique among charities in costing nothing in the working, and in being inaccessible to direct or indirect canvassing. The amount hitherto expended in relief since its establishmeut has been £2240, extended to fifty-two individuals or families.

The Gilchrist Trust.—One of the most munificent bequests ever made in the interest of science is that of the late Dr. Borthwick Gilchrist, a retired Indian Medical Officer, well-known as the author of the 'Grammar of Hindostani.'

Dr. Gilchrist was an intimate friend of Dr. Birkbeck, Joseph Hume, Sir John Bowring, and others of the advanced liberals of fifty years ago, and took part in the establishment of the "London University," now University College. He died in 1841, leaving his large fortune to be devoted, after his wife's death, to "the benefit, advancement, and propaga-

tion of education and learning in every part of the world, as circumstances permit," the Trustees having an "absolute and uncontrolled discretion" as to the mode of applying it. The income of the Trust, which is being gradually augmented by the sale of building-lots at Sydney, where Dr. Gilchrist had invested a considerable sum in the purchase of an estate with a view to its ultimate rather than its immediate productiveness, now amounts to about £4000 per annum. The Trustees have created various Scholarships for bringing young men of ability from India and the Colonies to carry on their education in this country; and they have also given assistance to various educational institutions which they considered to have a claim for occasional help from the Fund, such as the Working Men's College in London and the Edinburgh School of Arts; and they have instituted short courses of scientific lectures to working men in London, Manchester, Leeds, and Liverpool.

The Trustees now desire to do something effectual for the *advancement* of learning; and a scheme—subsequently submitted to the Council of the Royal Society—was suggested by Dr. Carpenter, the Secretary of the Trust, as one which seemed to him to be the most effectual for carrying out this object; and it was adopted by the Trustees on his recommendation.

In a letter addressed to myself in June last Dr. Carpenter informed your Council that the Trustees of the Fund had resolved to employ a portion of it in the promotion of scientific research, and empowered him to submit the following liberal proposal to the consideration of your Council: namely, the Trustees propose annually to entertain the question of placing £1000 at the disposal of the Council of the Royal Society to be expended in grants to men of proved ability in scientific research, but who, from their limited pecuniary means, are precluded from prosecuting inquiries of great interest by the necessity of devoting to remunerative work the time they would wish to devote to such inquiries, the Council of the Society to undertake on their part to recommend to the Trustees suitable subjects of inquiry, competent men circumstanced as indicated, and the sum to be assigned in each case. The Trustees desire, further, that the grants should not be regarded as eleemosynary, but rather as Studentships carrying with them scientific distinction, and not as rewards for past work, but as means for work to be done.

Upon this communication (in which you cannot fail to perceive not only an enlightened regard for the interests of science on the part of the Trustees, but, on the part of their Secretary, an accurate perception of the best means of supplying one of the greatest scientific needs), your Council appointed a Committee to report on the proposal. Their labours are already concluded; the proposition has been accepted, but under stipulation for fulfilment of the following conditions by applicants for the grants:—

That the grants should be made for one year only in each case, though subject to renewal.

That the recipients be designated *Gilchrist Students* for the year in which the grants are made.

That no application for grants be received except it has been approved by the President and Council of any one of the six Societies—namely, the Royal, Astronomical, Chemical, Linnean, Geological, and Zoological; and that all applications be submitted to a Committee, consisting of the Presidents of the six Societies together with the Officers of the Royal Society, which Committee shall recommend the applicants to the Gilchrist Trustees.

That a form of application be prepared setting forth the general objects of the Gilchrist Studentships, and the conditions upon which they are conferred.

That each Student furnish, at the end of the year for which the grant is made, a report of his progress and results, signed by himself and countersigned by the President of the Society through which the application was transmitted.

Simple and acceptable as such a scheme appears, it may prove by no means always smooth in the working. It will be easy to find subjects, and candidates too; but the Trustees must not expect in every case a full annual harvest for what they annually sow, or that some of the seed will not be productive of a crop of good intentions rather than good fruits. Putting aside all the temptations to procrastination that prepayment fosters, there is the fact that every subject of scientific research presents a labyrinth in which the investigator may wander further and further from the main gallery, always following some tempting lateral track leading to discovery, but never either reaching the end of it or getting back to that which he set out to follow.

We must, however, hope for the best results from so munificent an endowment of scientific research, and watch with the deepest interest the progress of an experiment, the means for instituting which, after being urgently called for from the Government and our Universities, are now forthcoming from private resources.

The Wintringham Bequest.— Hitherto this curious bequest has, so far as the Society is concerned, proved alike profitless and troublesome, as will appear from a few particulars of its history.

Sir Clifton Wintringham, Bart., a Fellow and son of a Fellow of this Society, died at Hammersmith, January 10, 1794, and bequeathed £1200 three-per-cent. Consols (payable twelve months after the decease of his wife) to the Royal Society, subject to the condition that within one month of the payment of the annual dividends in each year the President should fix on the subjects for three essays in Natural Philosophy or Chemistry, and submit them to the Society to be adopted by secret ballot. The subjects were then to be advertised in the papers of London, Paris, and the Hague: the essays were to be sent to the Royal Society within ten months of date of advertisement, each author to deliver ten copies; and

the President and nine Members of Council were to choose the best, and then to have made a silver cup of £30 value, to be presented to the successful essayist on the last Thursday in December. In case of failure the dividends were to be paid to the Treasurer of the Foundling Hospital.

Lady Wintringham died in 1805; but the Royal Society heard nothing of the bequest until 1839, when steps were taken to obtain possession of the fund. The Foundling Hospital put forward their claim; legal proceedings were taken, costs being paid out of accumulated dividends; and in 1842 the Royal Society were put in possession of the £1200 stock. Owing to the essential difficulties of carrying out the conditions of the testator's will, the dividends have ever since been paid to the Foundling Hospital.

The Council, desirous that those difficulties should be overcome, have at different times appointed a Committee to examine the question and suggest if possible a solution; but no satisfactory conclusion has yet been arrived at.

The Handley Bequest.—Mr. Edwin Handley, of Old Bracknell, Berks, was a country gentleman, and the possessor of a considerable landed and personal estate in Berkshire and Middlesex. He died in 1843, having bequeathed the bulk of his property, after the decease of his two sisters, to the Royal Society.

The last of these ladies died in 1872, since when certain legal formalities have been complied with, and the claims of the Royal Society to the landed estates under the Mortmain Act have been brought before the Court. In February last the Master of the Rolls decided that "the gifts to the Royal Society, so far as they relate to pure personalty, are good charitable gifts, but otherwise void." The personalty as set forth in the "Bill of Complaint," comprises £6033 7*s.* 5*d.* Three-per-Cent. Consols, £1904 17*s.* 2*d.* Reduced, and £41 18*s.* 5*d.* Bank-of-England Stock.

By the terms of the Will the Society is to preserve the property intact in value, as a Fund Principal, the income of which is to be applied to the rewarding inventions in art, discoveries in science, physical or metaphysical ("which last and highest branch of science," to quote the testator's words, "has been of late most injuriously neglected in this country"), or for the assistance of fit persons in the prosecution of inventions and discoveries. The rewards or assistance are to be granted annually, or after longer periods, to British subjects or foreigners, according to the impartial decision of the President and Council.

The Dircks Bequest.—Mr. Henry Dircks, of Liverpool, and latterly of London, who died in 1872, has bequeathed the residue of his property (about £4000), after payment of debts and charges, to the Royal Society, Royal Society of Literature, Chemical Society, and Royal Society of Edinburgh, in equal shares and proportions, in furtherance of their

several objects. As, however, it is possible that certain claims to the residue under the Bankruptcy Act, dating from 1847, may be set up, we are advised that the estate cannot be administrated without the aid of the Court of Chancery, which has been appealed to accordingly.

The Ponti Will.—Lastly, it is my duty under this head to inform you that our Secretary has received a communication from the Secretary of State for Foreign Affairs, to the effect that the late M. Girolamo Ponti, of Milan, has bequeathed a portion of his immense property to the "Academy of Science of London." As, however, it does not appear what Society is indicated under this title, and as the relatives of the testator intend to dispute the Will, the Council, as at present advised, will take no steps in the matter. I have further to observe that under the terms of the Will, the Academy of Science will, if it accepts the trust, be burthened with annual duties and responsibilities respecting the distribution of the proceeds which would be altogether inconsistent with the position and purposes of the Royal Society.

The Fairchild Lecture.—This Lecture no longer appears in the annual financial statement of your Treasurer. Though an obvious anachronism and regarded almost from the first with little sympathy either within or without our walls, it should not pass away without a notice from the Chair. In February 1728 Thomas Fairchild, of Hoxton, gardener, bequeathed £25 to be placed at interest for the payment of 20*s.* annually for ever for preaching a sermon in the parish church of St. Leonard's on Tuesday in Whitsun week on "the wonderful works of God in the creation, or on the certainty of the resurrection of the dead proved by certain changes of the animal and vegetable parts of the creation." From 1733 to 1758 most of the lectures were read by Archdeacon Denne, one of the original Trustees, who in 1746 contributed all his lecture-fees to the fund, which, with a subscription raised by the Trustees, enabled them in 1746 to purchase £100 South Sea stock. Subsequently this stock was offered to and accepted by the Society: the transfer was made in 1767; and from that date the Lecturers were appointed by the President and Council. The lectures have been regularly delivered, but of late years to empty pews, under which circumstances the Council, after full deliberation, unanimously resolved that it was desirable to relieve the Society from the Fairchild Trust, and that to this end application should be made to the Charity Commissioners. The regular forms having been gone through, the Trust was transferred to the Commissioners in November last, and thus disappears from our balance-sheet.

The *Croonian* and *Bakerian* Lectures are given annually as usual; and those of this year appear in our Proceedings. These do not diminish in interest and importance.

The Davy Medal.—The Council has accepted the duty of annually awarding a medal, to be called the Davy Medal, for the most important

discovery in Chemistry made in Europe or Anglo-America. The history of this medal is as follows :—

Our former illustrious President, Sir Humphry Davy, was presented by the coal-owners of this country with a service of plate, for which they subscribed £2500, in recognition of his merits as inventor of the Safety Lamp. In a codicil to his will Sir Humphry left this service of plate to Lady Davy for her use during her life, with instructions that after her death it should pass to other members of the family, with the proviso that, should they not be in a situation to use or enjoy it, it should be melted and given to the Royal Society, to found a Medal to be awarded annually for the most important discovery in Chemistry, anywhere made in Europe or Anglo-America.

On Sir Humphry's death the service of plate became the property of his brother, Dr. John Davy, F.R.S., who, in fulfilment of Sir Humphry's intentions, bequeathed it after the death of his widow, or before if she thought proper, to the Royal Society, to be applied as aforesaid. On the death of Mrs. Davy the plate was transferred to the custody of your Treasurer, and, having been melted and sold, realized £736 8*s.* 5*d.*, which is invested in Madras guaranteed railway stock, as set forth in the Treasurer's balance-sheet. The legacy duty was repaid to the Society by the liberality of the Rev. A. Davy and Mrs. Rolleston.

The style and value of the medal, and the steps to be taken in reference to its future award, are now under the consideration of the Council, and will, I hope, be laid before you on the next Anniversary. The acceptance of the trust has not been decided upon without long and careful deliberation, nor without raising the question of the expediency of recognizing scientific services and discoveries by such trivial awards as medals, and of the extent to which the awards entrusted to our Society are depreciated by their multiplication. My own opinion has long been that some more satisfactory way of recognizing distinguished merit than by the presentation of a medal might be devised, and that the award might take a form which would convey to the public a more prominent and a more permanent record of the services of the recipients, such as a bust or a portrait to be hung on our walls, or a profile or a record of the discovery to be engraved on the medal, which might be multiplied for distribution or sale to Fellows and to foreign Academies. In short, I consider awards of medals without distinctive features to be anachronisms; it is their purpose, not their value, which should be well marked; and the question is, whether that purpose is well answered by their being continued under the present form.

Instruments.—The small but remarkable and, indeed, classical collection of instruments and apparatus belonging to the Society, and for which there was no accommodation in old Burlington House, was, on our migration from Somerset House in 1857, by order of the Council, deposited in the Observatory in the Kew Deer-Park, near Richmond, then under the control of the British Association.

The instruments have been now for the most part brought back and placed in our Instrument-Room, and will, I hope, at no distant period be accessible to the Fellows.

On the motion of General Smythe, seconded by Mr. Francis Galton, it was resolved—"That the thanks of the Society be returned to the President for his Address, and that he be requested to allow it to be printed."

I now pass to the presentation of the Medals.

The Copley Medal has been awarded to Prof. Louis Pasteur, one of our Foreign Members, "for his researches on Fermentation and on Pebrine."

Prof. Pasteur's researches on fermentation consist essentially of two parts:—the first part, in which he enters exhaustively into the examination of the products formed in this process; and the second, in which he takes up the question of the cause of fermentation.

Previous observers had noticed the production, in solutions of sugar which had been fermented, of substances other than the two commonly recognized, alcohol and carbonic acid; but it remained for Pasteur to show which were essential, and which were occasional products. In the series of able papers contributed to the 'Comptes Rendus' and to the 'Annales de Chimie et de Physique,' he proved conclusively that succinic acid and glycerine were always found in fermented solutions of sugar, while lactic acid and acetic acid, although occasionally present, were not always so. He also showed that, in addition to these substances, a part of the sugar was converted into cellulose and fat.

The study of the products formed during fermentation opened the way to the second part of the research, viz. the cause of fermentation.

It had been found that certain solutions, when exposed to the air, soon became full of living organisms; and Pasteur's experiments led him to support the view that these organisms originated from the presence of germs floating in the air. He found that no living organisms were developed if care were taken to destroy completely all those which might be present in the solution, and if the solutions were then carefully sealed up free from air. Nor was it necessary to exclude the air, provided that pure air, free from germs, were admitted. By passing the air through red-hot tubes or through gun-cotton before reaching the solutions, he found that the development of organisms, in such boiled solutions, did not take place. An exception to this was noticed in the case of milk, which required to be heated to a higher temperature than the boiling-point of water at atmospheric pressure. Pasteur showed that this was connected with the alkaline reaction of milk, for in all cases in which the development of life was prevented by heating to the boiling-point of water, the solutions

had a faintly acid reaction—but that when this was neutralized by carbonate of lime, the solutions then behaved like milk.

Prof. Pasteur also examined the gun-cotton through which the air has been passed; and he found, among other things, certain cells to which he attributed the power of causing the growth of organisms in solutions. By sowing some of these cells in solutions which previously had remained clear, and finding that such solutions speedily became turbid from the growth of living organisms, it was proved that the air which had passed through the gun-cotton had lost its property of causing the development of life in solutions because the germs which the air contained had been stopped by the gun-cotton.

The result of the second part of the research may be thus summed up:—

1. No organisms are developed in solutions if care be taken to prevent the possibility of the presence of germs.

2. This negative result does not depend upon the exclusion of oxygen.

3. The matter separated from ordinary air is competent to develop organisms in solutions which previously had remained unchanged.

Not less important were the results of Pasteur's experiments respecting the chemical functions of the ferment.

It had been held that the entire ferment was in a state of putrefactive decomposition, and induced a similar decomposition in the sugar with which it was in contact.

In corroboration of this view, it was stated that ammonia (a product of the decomposition of albuminous substances such as those present in the ferment) is always found in liquids which are undergoing fermentation.

Pasteur proved that the ammonia in fermenting liquids diminishes in quantity in proportion as the process advances, and that the yeast-cells increase and grow while forming complex albuminous substances at the expense of the ammonia and other aliments which are supplied to it. He found that, in addition to ammonia and sugar, the cells require mineral substances, such as phosphates and other constituents, such as are present in the organism of every healthy and growing yeast-cell.

In short, he proved that those conditions which are most favourable to the healthy growth and development of the yeast-cells are most conducive to the progress of fermentation, and that fermentation is impeded or arrested by those influences which check the growth or destroy the vitality of the cell.

The above results are but samples of the fruits of Pasteur's long series of researches in this subject. Many and many an able investigator had worked in the same field; and such were the difficulties they encountered, that Dumas himself recommended Pasteur not to waste his time in working at so hopeless a subject.

To the biologist, two of Pasteur's researches are of very great interest and importance. He has shown that *Fungi* find all the materials needed for their nutrition and growth in water containing an ammonia salt and

certain mineral constituents, and devoid of any nitrogenized organic matter; and he has proved that all the phenomena presented by the destructive silk-worm epidemic, the *pebrine* (even the singular fact that it is hereditarily transmitted through the female and not through the male), are to be explained by the presence of a parasitic organism in the diseased caterpillars.

The medal was received for Prof. Pasteur by the Foreign Secretary of the Society.

The Rumford Medal has been awarded to Mr. J. Norman Lockyer, F.R.S., "for his Spectroscopic Researches on the Sun and on the Chemical Elements."

Mr. Lockyer has long been engaged in spectroscopic researches on the sun. His first observations were directed to a scrutiny of the spectrum of sun-spots as compared with that of the general surface, with a view to bring evidence to decide between two rival theories respecting their formation. In the course of the paper in which his first observations were described, and which was read before the Royal Society on November 15th, 1866, he asks, "May not the spectroscope afford us evidence of the existence of the 'red flames' which total eclipses have revealed to us in the sun's atmosphere, although they escape all other modes of examination at other times?"

The spectroscope he then employed proved to be of insufficient dispersive power for his researches, and he was induced to apply to the Government-Grant Committee of the Royal Society for aid to construct one of greater power. This aid was accorded, and the instrument was delivered, though not quite complete, on the 16th of October, 1868. On the 20th his efforts were crowned by the detection of a solar prominence by means of the bright lines exhibited in its spectrum. An account of this discovery was immediately communicated to the Royal Society and to the French Academy of Sciences.

Meanwhile had occurred the total solar eclipse of August 18th, 1868, to observe which various parties had gone out armed with suitable instruments, and especially with spectroscopes, for determining the character of the hitherto unknown spectrum of the prominences; and the firstfruits of their labours had reached Europe, showing that the spectrum in question is one of bright lines. It occurred to Mr. Janssen, who had observed with eminent success the spectrum of the prominences during the eclipse, that the same mode of observation might enable one to detect them at any time, and he saw them in this manner the very next day. The first account of this discovery, which was sent by post, did not, however, reach the French Academy until a few days after the communication of Mr. Lockyer's notice; so that nothing interferes with the perfect independence with which these two physicists established the possibility of detecting the prominences at any time.

A discovery like this opened up a new field of research, which Mr. Lockyer was not backward in exploring. One of the firstfruits of the application of the method was the discovery of a continuous luminous gaseous envelope to the sun, which he calls the chromosphere, of which the prominences are merely local aggregations. Evidence was further obtained of gigantic convulsions at the surface of the sun, which were revealed by slight alterations of refrangibility in the lines, observed in a manner similar to that in which Mr. Huggins had determined the relative velocity of approach or recess of the Earth and Sirius.

The interpretation of spectroscopic solar phenomena required a reexamination in several respects of the spectroscopic features of artificial sources of light. Among these researches special mention must be made of Mr. Lockyer's classification of the lines due to the metals of the electrodes between which an induction discharge was passed, according to their "length," *i. e.* the distance from the electrodes to which they could respectively be traced. This led to the explanation of various apparent anomalies as to the presence or absence of certain dark lines in the solar spectrum, and to the detection of additional elements in the sun, especially potassium, an element which, though so common on the earth and so easily detected by spectral analysis, had not previously been proved to exist in the sun, because the attention of observers had been turned in a wrong direction, as was shown by these researches.

Nor was it only in relation to solar physics that these researches bore fruit. They led to a *quantitative* determination in many cases, by means of the spectroscope, of the proportion of the constituents in an alloy, and afforded new evidence of the extent to which impurities are present even in substances deemed chemically pure.

The Medal was received by Mr. Lockyer.

A Royal Medal has been awarded to Mr. Henry Clifton Sorby, F.R.S., "for his Researches on Slaty Cleavage and on the minute Structure of Minerals and Rocks; for the construction of the Micro-Spectroscope, and for his Researches on Colouring-matters."

The principal grounds on which Mr. Sorby's claims to a Royal Medal rest are the following:—

1. His long-continued study, and his successful application of the microscope to the solution, of problems in petrology.

2. His employment of the prism in conjunction with the microscope for the analysis of the colours transmitted by substances, as well organic as inorganic.

Though Mr. Sorby's labours during the last ten years have been more particularly devoted to observations of the latter class, his work, extending over a period that commenced in 1849, is represented in the Catalogue of Scientific Papers (limited by the year 1863) by no less than 47 memoirs. Among the more remarkable of these must be mentioned the

Reports to the British Association and the contributions to the Philosophical Magazine (1853, 1856, 1857), in which he grappled with the subject of slaty cleavage, and helped to establish the explanation that cleavage was the result of greater relative condensation of the material in a direction perpendicular to the cleavage, due in the case of rocks to mechanical compression in that direction—an idea that met with immediate illustration from other experimentalists.

His memoirs on the temperatures and pressures at which certain rocks and minerals were formed (in the Geological Society's Journal, 1858), founded on the relative volume of the liquid and vacuous portions of microscopic hollows, or, again, on the character of microscopic substances mingled with the mineral matter he investigated, convinced the geologist that he had to take into account the action of water under high pressures and at high temperatures in explaining the formation of granitoid rocks.

And the refinement of the methods that Mr. Sorby employed for making his rock-sections at Sheffield has made those methods the models sought after by the now large school of Continental and English microscopic petrologists.

His applications of spectroscopic methods to the microscope fall more strictly within the limit of ten years, as they have been worked out since 1867, when Mr. Sorby first described his adaptation of the spectroscope to the microscope, as carried out by Mr. Browning.

The observations he has made with this instrument, and generally by combining optical examination with the use of chemical reagents, have extended over a very wide range—such as the recognition of blood-stains, of adulteration in wine, the means of discriminating among the compounds of certain of the metals, chiefly of zirconium, titanium, and uranium, by the aid of blowpipe-beads—and finally to the elucidation, to a considerable extent, of the causes of the complexity in the tints exhibited by plants in the different stages of development of their annual foliage and flowers.

These are only some of the more important of Mr. Sorby's contributions to science; and they are characterized by an untiring application of the methods of experimental research to a great variety of subjects suggested by a very ingenious and active mind.

The Medal was received by Mr. Sorby.

A Royal Medal has been awarded to Prof. William Crawford Williamson, F.R.S., "for his Contributions to Zoology and Palæontology, and especially for his Investigation into the Structure of the Fossil Plants of the Coal-Measures."

Professor Williamson's contributions to Biological Science were commenced forty years ago, and embrace investigations into the structure of the Foraminifera, the Rotifera, the scales and bones of Fishes, and the fossil plants of the Carboniferous and Oolitic periods. These comprise works of great merit and value, not only on account of their accuracy

and the extent and novelty of the observations which they contain, but by reason of the breadth of view and the philosophical spirit which pervade them.

His labours in Vegetable Palæontology are above all remarkable, being alike laborious, searching, and productive of important results. These are embodied in six contributions (of which the last will soon appear) to the Philosophical Transactions upon the organization of the Fossil Plants of the Coal-measures—and one on the restoration of a Cycadeous tree (*Zamia gigas*) from the Yorkshire Oolite, published in the Transactions of the Linnean Society. These are not only models of laborious research and exact description, but they are illustrated by more than fifty plates, devoted to microscopic analyses of vegetable tissues, obtained by making transparent slices of the fossils. Both the slices and the drawings are made by Prof. Williamson himself, who thus, to his reputation as a biologist, unites those of an accomplished artist and a skilful lapidary, qualifications which should be named along with those for which the medal is awarded, because no unscientific lapidary could have obtained equally illustrative sections, and no common artist could have depicted them with equal exactitude. The more important results thus obtained refer to the structure, affinities, and reproductive organs of *Calamites* and its allies, to *Lepidodendron*, *Sigillaria*, *Lepidostrobus*, *Asterophyllites*, and to other genera of the Carboniferous epoch.

In addition to these contributions to the history of previously known genera of that epoch, Prof. Williamson has been able to show, on the one hand, that groups of now living plants which were not previously supposed to have a great geological antiquity, actually flourished during the Carboniferous period, and, on the other, that plants of that period which had been previously referred with confidence to groups now living, have in reality other and widely different affinities.

The Medal was received by Prof. Williamson.

The Statutes relating to the election of Council and Officers having been read, and Mr. A. J. Ellis and Col. Strange having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were collected, and the following were declared duly elected as Council and Officers for the ensuing year:—

President.—Joseph Dalton Hooker, C.B., M.D., D.C.L., LL.D.

Treasurer.—William Spottiswoode, M.A., LL.D.

Secretaries.— $\begin{cases} \text{Prof. George Gabriel Stokes, M.A., D.C.L., LL.D.} \\ \text{Prof. Thomas Henry Huxley, LL.D., Ph.D.} \end{cases}$

Foreign Secretary.—Prof. Alexander William Williamson, Ph.D.

Other Members of the Council.—Prof. J. Couch Adams, LL.D.; the Duke of Devonshire, K.G., D.C.L.; Capt. Frederick J. O. Evans, R.N.,

C.B.; John Evans, Pres. G.S., F.S.A.; Albert C. L. G. Günther, M.A., M.D.; Daniel Hanbury, Treas. L.S.; Sir John Hawkshaw, Knt., M.I.C.E.; Joseph Norman Lockyer, F.R.A.S.; Robert Mallet, C.E., M.R.I.A.; Nevil Story Maskelyne, M.A.; C. Watkins Merrifield, Hon. Sec. I.N.A.; Prof. Edmund A. Parkes, M.D.; Right Hon. Lyon Playfair, C.B., LL.D.; Andrew Crombie Ramsay, LL.D.; Major-Gen. Sir. H. C. Rawlinson, K.C.B.; Prof. J. S. Burdon Sanderson, M.D.

The thanks of the Society were given to the Scrutators.

The following Table shows the progress and present state of the Society with respect to the number of Fellows:—

	Patron and Royal.	Foreign.	Compounders.	£4 yearly.	Total.
December 1, 1873.	4	48	266	258	576
Elected			+ 6	+ 10	+ 16
Deceased		– 6	– 7	– 7	– 20
Since compounded .			+ 2	– 2	
Withdrawn			– 1		– 1
	4	42	266	259	571

Statement of Income and Expenditure during the Year ending November 30, 1874.

	£	s.	d.
Annual Contributions	1059	0	0
Admission Fees	160	0	0
Compositions	400	0	0
Rents	325	5	9
Dividends (exclusive of Trust Funds)	1525	0	10
Acton Estate, Sale of Land	100	0	0
Sir F. Ronalds, Bequest (less Legacy Duty)	450	0	0
Sale of Transactions, Proceedings, &c.	636	17	10
Royal Academy, Fixtures	52	0	0
Repayments	12	9	11
	£4720	14	4

	£	s.	d.	£	s.	d.
Salaries and Wages				1177	16	3
Ronalds Bequest. Bought £484 10*s.* 5*d.* Consols				450	0	0
Acton Land Sale. Bought £107 4*s.* 9*d.* Consols				100	0	0
The Scientific Catalogue				225	3	0
Books for the Library				165	16	8
Binding ditto				113	14	11
Printing Transactions, Parts I., II. 1873, Part I. 1874	589	0	9			
Ditto Proceedings, Nos. 147–155	377	3	5			
Ditto Miscellaneous	81	6	6	2081	2	1
Paper for Transactions and Proceedings	215	1	0			
Binding and Stitching ditto	116	16	9			
Engraving and Lithography	701	13	8			
Soirée Expenses				50	11	5
Removal Charges, Fittings and Repairs	509	12	1			
Miscellaneous Expenses	30	9	2			
Coal, Lighting, Soap, &c.	139	0	5			
Tea Expenses	22	13	6			
Fire Insurance	20	1	6			
Taxes	6	5	0	815	0	3
Advertising	14	0	6			
Few and Co., Law Charges	13	5	4			
Smith, Son, and Oakley, Surveying	11	6	6			
Postage, Parcels, and Petty Charges	46	4	3			
Mablethorpe Schools, Donation	2	2	0			
				£5179	4	7

Trust Funds.

	£	s.	d.	£	s.	d.
Donation Fund	164	11	2			
Rumford Fund	68	19	2			
Wintringham Fund	35	12	6	311	15	6
Copley Medal Fund	9	19	7			
Davy Medal Fund	32	13	1			
Balances from 1873				693	13	5
				£5726	3	3

	£	s.	d.	£	s.	d.
Donation Fund	225	0	0			
Wintringham Fund	35	9	6			
Copley Medal Fund	4	18	4	272	6	10
J. N. Lockyer, Bakerian Lecture	4	0	0			
Dr. Ferrier, Croonian Lecture	2	19	0			
				5451	11	5
Balance at Bank				236	18	8
Balances on hand, Catalogue and Petty Cash				37	13	2
				£5726	3	3

W. SPOTTISWOODE,

Treasurer.

Estates and Property of the Royal Society, including Trust Funds.

Estate at Mablethorpe, Lincolnshire (55 A. 2 R. 2 P.), £136 per annum.
Estate at Acton, Middlesex (34 A. 2 R. 9½ P.), £170 per annum.
Fee Farm near Lewes, Sussex, rent £19 4*s.* per annum.
One fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, £3 per annum.
Stevenson Bequest. Chancery Dividend. One fourth annual interest on £85,336, Government Annuities and Bank Stock (produced £449 2*s.* 5*d.* in 1873–74).
£14,000 Reduced 3 per Cent. Annuities.
£30,161 0*s.* 10*d.* Consolidated Bank Annuities.
£403 9*s.* 8*d.* New 2½ per Cent. Stock—Bakerian and Copley Medal Fund.
£660 Madras Guaranteed 5 per Cent. Railway Stock—Davy Medal Fund.
£10,000 Italian Irrigation Bonds—The Gassiot Trust.

Trust Funds. 1874.

Scientific Relief Fund.

	£	s.	d.
Investments up to July 1872, New 3 per Cent. Annuities	6328	11	2
" " Metropolitan $3\frac{1}{2}$ Consols	100	0	0
	£6428	11	2

Dr.	£	s.	d.	*Cr.*	£	s.	d.
Balance	243	1	5	By Grants	220	0	0
Dividends	191	7	7	Balance	214	9	0
	£434	9	0		£434	9	0

Donation Fund.

£5816 1*s.* 1*d.* Consols.

	£	s.	d.		£	s.	d.
To Balance	680	15	2	By Grants	225	0	0
Dividends	164	11	2	Ronalds Bequest: bought £484 10*s.* 5*d.* Consols	450	0	0
Sir F. Ronalds, Bequest	450	0	0	Balance	620	6	4
	£1295	6	4		£1295	6	4

Rumford Fund.

£2322 19*s*. Consols.

	£	s.	d.		£	s.	d.
1874. Two years' Dividends	137	12	7	Medal Cases	1	10	0

Bakerian and Copley Medal Fund.

£403 9*s*. 8*d*. New $2\frac{1}{2}$ per Cent.

	£	s.	d.		£	s.	d.
To Balance	68	3	10	By Gold Medal	4	18	4
Dividends	9	19	7	Bakerian Lecture	4	0	0
				Balance	69	5	1
	£78	3	5		£78	3	5

Wintringham Fund.

£1200 Consols.

	£	s.	d.		£	s.	d.
To Balance, 1873	35	9	6	By Payment to Foundling Hospital, 1874	35	9	6
Dividends, 1874	35	12	6				
	£						

Croonian Lecture Fund.

	£	s.	d.		£	s.	d.
To one fifth of Rent of Estate at Lambeth Hill, payable by the College of Physicians	2	19	0	By Croonian Lecture	2	19	0

Davy Medal Fund.

£660 Madras Guaranteed 5 per Cent. Railway Stock.

	£	s.	d.		£	s.	d.
To Balance	136	11	1				
Dividends	32	13	1				
	£169	4	2				

The Gassiot Trust.

£10,000 Italian Irrigation Bonds.

	£	s.	d.		£	s.	d.
Dividends	498	18	4	By Payments to Kew Committee	498	18	4
Bonds drawn	469	0	0	Bonds bought	290	10	0
				Balance	178	10	0
	£967	18	4		967	18	4

Account of the appropriation of the sum of £1000 annually voted by Parliament to the Royal Society (the Government Grant), to be employed in aiding the advancement of Science (continued from Vol. XXII. p. 20).

1874.

1. Mr. J. W. L. Glaisher, for Stereotype Plates for a Table of the Power of Numbers	£100
2. Prof. Roscoe, for Instruments for Automatic Registration of Chemical Intensity of total Daylight	50
3. Mr. Schorlemmer, for Researches on the Hydrocarbons	50
4. Mr. Lockyer, for Spectroscopic Researches	200
5. Mr. Schäfer, for Investigation of Connective Tissue	50
6. Messrs. Miller and Skertchley, for Researches in Evaporation	50
7. Mr. A. H. Garrod, for an Investigation of the Movements of the Pulse	50
8. Mr. Crookes, for Researches on Attraction and Repulsion accompanying Radiation	100
9. Dr. Brunton, for Apparatus and Material to be used in an Experimental Investigation of the Physiological action of Ammonia	50
10. Dr. Klein, for expense of preparing Plates for his work on 'The Anatomy of the Lymphatic System'	50
11. Dr. Armstrong, for an Investigation of the Properties of the Derivatives of Phenol	50
12. Mr. Whitehouse, for Researches and Experiments in reference to a new Hygrometer	50
13. Captain Noble, for continuation (jointly with Mr. Abel) of Researches on Explosives	100
	£950 0 0

Dr.

	£	s.	d.
To balance on hand, Nov. 30, 1873	1047	18	9
To Grant from Treasury (1874)	1000	0	0
Repayments :—			
Prof. Stewart	4	1	4
Parker and Jones	21	5	0
Mr. Friswell	20	0	0
Interest	23	7	0
	£2116	12	1

Cr.

	£	s.	d.
By appropriations as above	950	0	0
Printing and Postal charges	8	12	6
Balance on hand, Nov. 30, 1874	1157	19	7
	£2116	12	1

Account of Grants from the Donation Fund in 1873–74.

	£	s.	d.
Mr. H. Willett, in aid of the Sub-Wealden Exploration Fund	50	0	0
Dr. Carpenter, for apparatus to demonstrate his Theory of Oceanic Circulation	25	0	0
Dr. Ferrier, for continuation of his Investigations into the Functions of the Brain	50	0	0
Dr. Dohrn, for the use of the Stazione Zoologica at Naples	100	0	0
	£225	0	0

Report of the Kew Committee for the Year ending October 31, 1874.

Magnetic Work.—The Magnetograph instruments were dismounted in January 1874 for the purpose of thorough examination and readjustment, as was announced in last Report. The necessity for this measure is obvious, when it is remembered that the instruments had been in unintermitted action for the period of fifteen years.

The scale-values were accordingly redetermined, and the instruments handed over to Mr. Adie for examination and repair. They were returned and remounted in May, but have not been set in continuous action as yet, inasmuch as it is intended that the automatic records should be suspended for the entire year, so as to commence a new series of observations with the year 1875. The cost of these operations has been £77 10*s.*

The monthly observations with the absolute instruments have been continued, as usual, by Mr. G. M. Whipple, who also takes charge of the general magnetic work, in which he has had the assistance, for the first part of the year of Mr. Cullum, and latterly of Mr. Power.

As regards the Magnetic Reductions, the Tabulations of Declination have been completed up to the end of 1873; and copies of the results to 1872 have been intrusted, for discussion, to the two Sergeants of the Royal Artillery, formerly in Sir E. Sabine's office at Woolwich, who have been in constant attendance at Kew since Nov. 1871. The Tabulations of Inclination and Horizontal Force have not been effected.

Magnetic data have been supplied to Dr. Wijkander, of Stockholm, in connexion with the Swedish Expedition to Spitzbergen, M. Diamilla Müller, of Florence, and Capt. Creak, R.N.

A Unifilar and Dip-circle have been repaired for use at the Observatory, and another pair of similar instruments have been lent to the Rev. S. J. Perry, F.R.S., for use at Kerguelen Island, on the Transit of Venus Expedition, as mentioned in last Report.

Meteorological Work.—The several automatic arrangements for recording respectively the Barometer, the Dry- and Wet-Bulb Thermometers, the Anemometer, and the Rain-gauge, have been maintained in constant action under the superintendence of Mr. T. W. Baker, assisted by Mr. Foster and Mr. Figg; and the daily standard eye-observations for control of the photographic records have been made regularly.

The instrumental traces with hourly tabulated values are sent monthly to the Meteorological Office as in former years. The Barograms and Thermograms are obtained in duplicate, and one copy is preserved at Kew. As regards the Anemograms and Hyetograms, the copy is obtained by the method of tracing.

In addition to the regular work of Kew as one of the self-recording Observatories in connexion with the Meteorological Office, the duty of examining and checking the work of all the seven Observatories of the same character has been carried on, in accordance with the method described in the Report of the British Association for 1869. This portion of the work has been performed by Messrs. Cullum, Hawkesworth, and Deane.

A series of investigations have been conducted with the view of testing the degree of accuracy attainable in the tabulation of the Thermograms by the process described in the British-Association Report just referred to. It has been found to be an improvement to set the glass tabulating-scale by means of fiducial lines traced on the Thermograms by photographic means, in preference to setting it, as heretofore, by standard readings. The great advantage derived from the new method is the discovery of "bagging" whenever it exists in the curves.

Electrometer.—The Self-recording Electrometer, which had been taken to Glasgow for alteration, as described in last Report, was returned by Mr. White in February, and was adjusted for action on March 10. It has since continued in satisfactory working order.

Photoheliograph.—A necessity for reexamining the measurements of the series of Kew sun-pictures having arisen, they have been retransferred to Kew by Mr. De La Rue, and their reexamination has been undertaken, at his expense, by Mr. Whipple, assisted by Mr. M'Laughlin, who has been temporarily engaged for this purpose.

The eye-observations of the sun, after the method of Hofrath Schwabe, have been made daily by Mr. Foster, when possible, as described in the Report for 1872, in order, for the present, to maintain the continuity of the Kew record of sun-spots.

Extra Observations.—The Committee, at the request of Prof. Roscoe, F.R.S., undertook to test for a year an instrument which he had devised for measuring the chemical intensity of daylight, as described in the 'Proceedings of the Royal Society,' vol. xxii. p. 158. The apparatus was completed for trial in September, but a few preliminary experiments showed that it required further adjustment; so that operations in this

direction are suspended for the present, to be resumed as soon as the instrument is in a satisfactory condition.

The daily record of temperature from Thermometers at different elevations on the Pagoda in the Royal Gardens, Kew, at the expense of the Meteorological Committee, was continued up to August, when it was interrupted, to be resumed during the winter months.

Verifications.—This department of the Observatory has exhibited increased activity, especially as regards the verification of Thermometers and the construction of Standard Thermometers.

The following magnetic instruments have been verified:—

Constants have been determined for

A Unifilar	for	Prof. J. Clerk Maxwell, F.R.S.
,,	,,	Prof. Balfour Stewart, F.R.S.
,,	,,	Rev. S. J. Perry, F.R.S.
,,	,,	Mr. P. Adie.
A Magnet	,,	Lisbon Observatory.
,,	,,	Prof. Buys Ballot, Utrecht.
,,	,,	Prof. Smirnow, Kasan.
3 Magnets	,,	Kew-Observatory stock.

The following instruments have been verified:—

2 Dip-circles	for	Mr. Casella.
1 Dip-circle	,,	Prof. Wild, St. Petersburg.
1 ,,	,,	The Imperial Admiralty, Berlin.
2 Fox's Circles	,,	,, ,, ,,
2 Needles	,,	Prof. Smirnow.
2 ,,	,,	Dr. E. van Rijckevorsel.
1 ,,	,,	H.M.S. 'Challenger.'

3 Azimuth Compasses for the Royal Geographical Society.

The complete set of Magnetographs for the Rev. A. M. Colombel, S. J. for Zi-ka-wei, near Shanghai, have been verified and forwarded to their destination.

A set of similar instruments has been ordered by Capt. Pujazon for the Marine Observatory of San Fernando, near Cadiz.

The part of this work which relates to Meteorology is entrusted to Mr. Baker. The meteorological instruments which have been verified are as follows:—

Barometers, Standards	110
,, Marine and Station	40
	150
Aneroids	10

Thermometers,	ordinary Meteorological	1471
„	Boiling-point Standards	22
„	Mountain	32
„	Clinical	1255
		2780

In addition, thirty-six Thermometers have been tested at the freezing-point of mercury, and one metallic Thermometer has been tested.

Eighteen Kew Standard Thermometers have been calibrated and divided at Kew.

The following miscellaneous instruments have also been verified:—

Rain-gauges	13
Robinson's Dial Anemometers	14
Telescope	1
Sextant	1
Theodolite	1
Hydrometers	66

A Barograph and Thermograph have been verified for Mr. Kingston for the Observatory at Toronto, and the values of the Scales have been determined as far as practicable.

Experiments have been made with a view to the construction of an apparatus devised by Mr. F. Galton, F.R.S., for facilitating the verification of thermometers, and also for rendering it possible to extend the range to which the Kew verifications at present apply.

A large stock of filled Thermometer-tubes for the construction of Standards has been laid in, and the tubes have been annealed.

In the last Report mention was made of certain experiments in progress with respect to the testing of Anemometers, a piece of ground having been rented in the Park for erecting the instruments.

The experience of a few months was sufficient to show that the exposure in the Park was not nearly sufficiently open to afford facilities for testing the instruments at any but very low velocities, and not very satisfactorily even in such cases. Application was therefore made to the Secretary of the Crystal Palace Company for permission to employ a rotary machine driven by steam-power, so as to be able to vary the velocities at pleasure.

Consent having been most freely given, the experiments were commenced, and the instruments tested at various velocities up to about 30 miles an hour, the highest attainable by the apparatus. The investigations were interrupted during the summer, and will be resumed on a future occasion. It is hoped that by this method of artificial rotation, which was that employed by Smeaton in his experiments on windmill sails, more satisfactory results will be attained than it is otherwise possible to

get. The expense of these experiments has been defrayed by a vote of the Government-Grant Fund.

The experiments on the vibration of pendulums, which were conducted by Capt. Heaviside, R.E., in connexion with the Great Trigonometrical Survey of India, as mentioned in the last Report, were completed at the end of May. The apparatus employed in the experiments, with the exception of the Russian pendulums and their accessories, was, at the request of Prof. G. G. Stokes, F.R.S., received at Kew for storage.

The apparatus for testing Sextants, which had been temporarily removed in 1873 to afford space for swinging the Russian pendulums, has now been restored. The entire cost of this restoration and all other expenses connected with these pendulum experiments have been defrayed by the India Office.

The collimators of the Sextant Testing-apparatus have been arranged so as to be illuminated by gas-jets.

Waxed paper for photographic purposes has been supplied to the Meteorological Office (3 reams), the India Office (1 ream), the Magnetic Observatory, Toronto (4 reams), the Central Observatory, St. Petersburg (1 ream), the Hohe Warte, Vienna ($\frac{1}{2}$ ream), the Observatory of Don Luiz, Lisbon ($\frac{1}{2}$ ream).

It has been found necessary to make a change in the arrangements for obtaining waxed paper. For many years, through the kindness of a firm, the paper waxed at Kew has been hot-pressed at a nominal charge, but it was not found possible to continue this arrangement for an indefinite period. Waxed paper has now to be purchased, ready-made, at a considerable increase of cost, and the rate at which it is supplied to observatories has been consequently proportionately increased.

Mr. M'Clatchie and Mr. Beazeley, gentlemen holding appointments in the Chinese Customs Departments, and Mr. Steventon, appointed Assistant to the Observatory at Mauritius, have received instruction in the manipulation and methods of testing both Meteorological and Magnetical instruments, and the management of the photography of the self-recording apparatus.

In the month of September the Superintendent was informed by the President of the Royal Society that that body was prepared to remove the instruments belonging to it, which had been deposited at Kew for storage in Sept. 1851. Accordingly, on the 25th of September, they were handed over to Mr. Ladd, Optician, who had been commissioned by the Council to receive them.

The several pieces of Mechanical Apparatus, such as the Whitworth Lathe and Planing Machine, procured by Grants from either the Government-Grant Fund or the Donation-Fund, for the use of the Kew Observatory, have been kept in thorough order; and many of them are in constant and the others in occasional use at the Observatory.

Library.—In addition to the usual Donations of English and Foreign

Scientific Books, a most valuable present, consisting of twenty-four volumes, chiefly astronomical, has been received from the Athenæum Club.

Staff.—The Staff employed at Kew are as follows;—Mr. Samuel Jeffrey, Superintendent; G. M. Whipple, B.Sc., First Assistant; T. W. Baker, Second Assistant; J. E. Cullum, J. W. Hawkesworth, J. Foster, F. Figg, A. B. Deane, C. Power, and E. Constable.

Note.—In May Mr. Rigby, whose name appeared in the Report for 1873, resigned, and Mr. Power was appointed to fill the vacancy caused by the promotion of some of the Junior Assistants.

Mr. Robert H. Scott, F.R.S., continues to act as Honorary Secretary to the Committee.

Visitors.—The Observatory has been honoured during the year by the presence of several scientific men of distinction. Among these may be mentioned:—

M. A. d'Abbadie.
Mr. J. Allan Broun, F.R.S.
Mr. H. F. Blanford.
M. Marié Davy.
Prof. Buys Ballot.
M. W. de Fonvielle.
M. W. H. v. Freeden.
Capt. Hoffmeyer.
M. Le Verrier.
Dr. R. J. Mann, F.R.A.S.
M. H. Mohn.
Prof. A. Moritz.
Dr. H. A. Meyer.
M. Robert Müller.
Senor Jose Montojo.
Capt. Montojo.
Dr. Neumayer.
Capt. Pujazon.
Capt. Rikatcheff.
M. J. v. Rysselberghe.
Capt. Stempel.
Prof. Wiedemann.

Abstract. *Kew Observatory Revenue and Expenditure Account from November* 1, 1873 *to November* 1, 1874.

Dr. RECEIPTS.

		£ s. d.
To Balance from 1872–73		522 3 1
Royal Society (Gassiot Trust)	£248 18 8	
" "	249 19 8	
		498 18 4
Meteorological Committee Allowances	162 10 0	
" " "	162 10 0	
" " "	162 10 0	
" " "	162 10 0	
		650 0 0
Meteorological Committee, for Postages &c.	16 5 6	
" " Pagoda Observations	21 0 0	
Royal Society Government Grant for Anemometer Experiments per Mr. Scott	2 15 0	
		40 0 6
Payment for Instruments by Commission		260 7 5
Sale of Waxed Paper		85 12 4
Verification Fees, Magnetic Instruments	24 5 6	
" " Meteorological Committee	41 3 0	
" " Admiralty	11 0 0	
" " Opticians &c.	177 2 3	
		253 10 9
Sale of Standard Thermometers	8 9 0	
" Surplus Blank Forms	0 11 3	
Instruction Fees &c.	1 6 0	
		10 6 3
Mr. De La Rue for Sun-work		34 17 1
Payment for Copying Registers		6 9 7
Captain Heaviside for Pendulum Experiments		30 8 8
Sale of Photographic Residues		8 19 7
		£2401 13 7

EXPENDITURE. *Cr.*

		£ s. d.
By Salaries and extra work		1005 10 9
Rent of Land	£11 0 0	
Fuel and Gas	69 15 4	
Furniture and Fittings	37 3 2	
Chandlery &c.	25 0 6	
		142 19 0
Printing and Stationery	28 3 5	
Postages	9 10 7	
Messenger and Housekeeper	53 0 0	
Night Observations	9 5 6	
Porterage and Contingencies	28 19 2	
		128 18 8
Instruments purchased on Commission		168 7 6
Postages and Payments on behalf of Meteorological Committee	20 17 9	
Pagoda Observations	15 19 0	
Anemometer Experiments on behalf of Mr. Scott	18 14 9	
		55 11 6
Preparation and purchase of Waxed Paper		120 8 10
Chemicals	23 7 4	
Thermometers	22 6 0	
Ice	2 5 6	
Anemograph Sheets	2 10 0	
Repair of Instruments &c.	86 15 6	
Carpenters Work and Sundries	31 1 7	
		168 5 11
Sun-work expenses		37 12 5
Roscoe's Photometer Experiment Expenses		7 9 0
Pendulum Experiment		17 12 6
London and Westminster Bank	479 3 9	
Cash in hand	69 13 9	
Balance		548 17 6
		£2401 13 7

November 24, 1874. Examined, compared with the vouchers, and found correct. (Signed) R. STRACHEY, *Auditor.*

ASSETS.

	£ s. d.
By Balance as per Statement	548 17 6
Instruction Fees due	23 2 0
Verification Fees due	15 0 0
Standard Thermometers sold	15 0 0
Waxed Paper sold	9 10 0
" " in stock	47 10 0
Meteorological Committee Sundries	6 7 6
Government-Grant Fund for Anemometer Experiments	15 19 9
Standard Thermometers in stock (valued at)	118 10 6
	£799 17 3

LIABILITIES.

	£ s. d.
To Gas and Fuel	27 0 0
Tubes for Standard Thermometers	90 10 6
Chemicals	1 9 3
Instruments and Apparatus	2 10 0
Purchase of S. Fernando Magnetographs	100 0 0
Balance	578 7 6
	£799 17 3

Presents received, November 19, 1874.

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Graz:—Naturwissenschaftlicher Verein für Steiermark. Mittheilungen. Jahrgang 1873. 8vo. The Institution.

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Manchester:—Geological Society. Transactions. Vol. I.–XII. and XIII. Part 5. 8vo. *Manchester* 1841–74. The Society.

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Johnson (Rev. S. J.) Eclipses, past and future, with general hints for observing the Heavens. 12mo. *Oxford* 1874. The Author.

Lee (J. E.) Roman Imperial Profiles, being a series of more than One Hundred and Sixty Lithographic Profiles enlarged from Coins. 8vo. *London* 1874. The Author.

Lloyd (Rev. Dr. H.), F.R.S. A Treatise on Magnetism, General and Terrestrial. 8vo. *London* 1874. The Author.

Meunier (Stanislas) Cours de Géologie Comparée, professé au Muséum d'Histoire Naturelle. 8vo. *Paris* 1874. The Author.

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November 26, 1874.

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December 10, 1874.

JOSEPH DALTON HOOKER, C.B., President, in the Chair.

The President announced that he had appointed as Vice-Presidents:—

The Treasurer.
The Duke of Devonshire.
Mr. John Evans.
Right Hon. Lyon Playfair.
Dr. Sanderson.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Development of the Teeth of the Newt, the Frog, and certain Lizards." By CHARLES S. TOMES, M.A. Communicated by JOHN TOMES, F.R.S. Received July 23, 1874.

(Abstract.)

That the "papillary stage" of tooth-development could not be said to at any time exist either in the frog or in certain fish, was pointed out nearly twenty years ago by Professor Huxley, who, however, accepted, on the authority of Goodsir, the latter's theory of the process as true of Man and Mammalia. In more recent years Kölliker and Waldeyer have traced out the course of the development of teeth with great accuracy in Man and some other Mammalia, with the result of showing that the usually accepted views propounded by Goodsir and Arnold are not by any means an accurate representation of what takes place in them.

Since the date of the publication of Professor Huxley's paper, I am not aware that any thing has been published bearing upon the development of the teeth of Reptilia and Batrachia, save a paper by Dr. Lionel Beale upon the development of the teeth of the Newt, and a short and inconclusive paper by Santi Sirena; with the exception of the papers alluded to, the subject may be taken to stand in the position which it occupied at the time of the publication of Professor Owen's 'Odontography,' in which we are told that the teeth-germs of Reptiles and Batrachia never stop at the papillary stage, but that the primitive dental papilla sinks into the substance of the gum and becomes inclosed by a capsule.

The principal facts which my observations enable me to state are:—

That there is no such thing as a "dental groove" or "dental fissure" in the Batrachia and Sauria, but that the whole process takes place beneath an unbroken surface of epithelium.

That there is no such thing as a stage of "free papillæ," and consequently no sinking of papillæ into the gum and subsequent encapsulation of the same.

Instead of being formed in a "dental groove" the teeth are developed in a region which may be termed the area of tooth-development, varying in form and extent in different Reptilia, but agreeing in all in possessing the following characters:—

It is bounded on the one side by the teeth in place and the parapet of bone which carries them, and on the other, or inner, side by an exceedingly sharply defined boundary, consisting of dense connective tissue. At the surface, near where the functional tooth projects above the oral epithelium, it is narrow, but it expands as it passes more deeply below the surface. Within this area are developing tooth-sacs of different ages, the interspaces being occupied with a loose areolar tissue, differing in appearance from that which is seen outside the area, and appearing to be derived from portions of older tooth-sacs, which have not been entirely used up in the formation of the teeth.

The individual tooth-sacs are formed thus: an inflection of the cells of the oral epithelium, in section like a tubular gland, passes down along the inner side of the area above defined, until it reaches nearly to the level of the floor of the area. The depth to which it penetrates is considerable in many forms, *e. g.* in the Lizards, in which, therefore, this double layer of epithelial cells appears a mere line.

At the bottom of this inflection of epithelial cells the adjacent tissue assumes the form of a small eminence (without at first any visible structural alteration), while the epithelial process takes the shape of a bell-like cap over the eminence.

This epithelial inflection then goes to form the enamel-organ; the eminence becomes the dentine-organ.

Thus the enamel-germ is the first thing recognizable, and the presence of this ingrowth of epithelial cells seems to determine the formation of a dentine-organ at that particular spot which lies beneath its termination.

The enamel-organs, after they are fully formed, retain a connexion with epithelial cells, external to the ovoid or spherical tooth-sacs, at their summits; and the enamel-organs of successive teeth appear to be derived from the necks of those of their predecessors rather than from fresh inflections from the surface of the oral epithelium, though I am not sure that this is, in all instances, the case.

The tooth-sac of the newt is entirely cellular, and has no special investment or capsule; under pressure it breaks up and nothing but cells remain, as was noted by Dr. Lionel Beale.

That of the frog has an investment, derived in the main from what may be called the accidental condensation of the surrounding connective tissue, which is pushed out of the way as it grows; while in the lizard

the base of the dentine-germ furnishes lateral prolongations, just as has been observed to be the case in man.

The dentine-organs conform closely with those of mammals; the odontoblast layer is very distinct, and the processes passing from these cells into the dentine-tubes are often visible.

The enamel-organs consist only of the outer and inner epithelia, without any stellate intermediate tissue; as, in some instances, enamel is certainly formed, the existence of the stellate tissue is obviously non-essential. When a tooth is moving to displace its predecessor, its sac travels with it, remaining intact until the actual attachment of the tooth to the bone by ankylosis.

II. "On the Structure and Development of the Teeth of Ophidia." By Charles T. Tomes, M.A. Communicated by John Tomes, F.R.S. Received October 5, 1874.

(Abstract.)

Contrary to the opinion expressed by Professor Owen and endorsed by Giebel and all subsequent writers, the author finds that there is no cementum upon the teeth of snakes, the tissue which has been so named proving, both from a study of its physical characters and, yet more conclusively, from its development, to be enamel. The generalization that the teeth of all reptiles consist of dentine and cement, to which is occasionally added enamel, must hence be abandoned.

Without as yet pledging himself to the following opinion, the author believes that in the class of Reptiles the presence of cementum will be found associated with the implantation of the teeth in more or less complete sockets, as in the Crocodiles and Ichthyosaurs.

The tooth-germs of Ophidia consist of a conical dentine-germ, resembling in all save its shape that of other animals, of an enamel-organ, and of a feebly expressed capsule, derived mainly from the condensation of the surrounding connective tissue.

The enamel-organ consists only of a layer of enamel-cells, forming a very regular columnar epithelium, and of a few compressed cells external to this, hardly amounting to a distinct layer; the enamel-organ is coextensive with the dentine-germ. There is no stellate reticulum separating the outer and inner epithelia of the enamel-organ.

The successional teeth are very numerous, no less than seven being often seen in a single section; and their arrangement is peculiar, and quite characteristic of the Ophidia.

The tooth next in order of succession is to be found at the inner side of the base of the tooth in place, where it lies nearly horizontally; but the others stand more nearly vertically, parallel with the jaw and with the tooth in place, the youngest of the series being at the bottom.

The whole row of tooth-sacs is contained within a single general connective-tissue investment, which is entered at the top by the descending process of oral epithelium, whence the enamel-germs are derived.

As they attain considerable length, the forming teeth, which were at first vertical, become nearly horizontal, resuming, of course, their upright position once more when they come into place.

The clue to the whole peculiarity of this arrangement is to be found in the extreme dilatation which the mouth of the snake undergoes. The general capsular investment probably serves to preserve the tooth-sacs from displacement; while, if the forming teeth remained vertical after they had attained to any considerable length, their points would be protruded through the mucous membrane when this was put upon the stretch in the swallowing of prey.

Just as the author has shown in a previous communication to be the case in the Batrachia and Sauria, the hypothetical "papillary stage" is at no time present.

From the oral epithelium there extends downwards a process which, passing between and winding around the older tooth-sacs, after pursuing a tortuous course, reaches the furthest and lowest extremity of the area of tooth-development. Here its cæcal end gives origin to an enamel-organ, and, while it does so, buds forth again beyond it in the form of a cæcal extremity. Thus at the bottom of this area of tooth-development there is a perpetual formation of fresh enamel-organs, beneath which arise corresponding dentine-organs, or papillæ, if such they can be called when arising thus far away from the surface.

In essential principle, therefore, the formation of a tooth-germ is similar to that already described in mammals aud other reptiles, the difference lying principally in the enormous relative length of, and the tortuous course pursued by, that inflection of the oral epithelium which serves to form the enamel-organs. The attachment of the tooth to the jaw is effected by the rapid development of a coarse bone, which is not derived from the ossification of the feebly expressed tooth-capsule, but from tissues altogether external to it. Nevertheless this coarse bone of attachment adheres more closely to the tooth than to the rest of the jaw, from which, in making sections, it often breaks away.

The base of the dentinal pulp assists in firmly binding the tooth to this new bone, being converted into a layer of irregular dentine.

This "bone of attachment" is almost wholly removed and renewed with the change of each tooth.

III. "On the Effects of Heat on Iodide of Silver." By G. F. RODWELL, F.R.A.S., F.C.S., Science Master in Marlborough College. Communicated by Professor FREDERICK GUTHRIE, F.R.S. Received August 14, 1874.

Professor Clerk Maxwell, when discussing the expansion of matter by heat ('Theory of Heat,' p. 8), says, "The body generally expands (the only exception among solid bodies, as far as I am aware, is the iodide of silver, which has been found to contract as the temperature rises)." M. H. Fizeau, speaking of the same substance ('Nouvelles Observations relatives à l'iodure d'argent'), writes as follows :—" Ce corps, en effet, paraît offrir l'exemple d'une inversion complète des phénomènes ordinaires de la dilatation par la chaleur, car son volume diminue très-certainement pendant l'échauffement et augmente pendant le refroidissement."

It was thought that a substance possessing so marked a property would probably exhibit peculiarities of molecular structure; and the following experiments were made in order to determine whether such peculiarities exist, to note the effects of higher temperatures upon the iodide than those employed by Fizeau (which in no instance exceeded 100° C.), and to determine the point of maximum density of the iodide. The phenomena which most closely approximate to those assigned to the iodide of silver when heated are to be found in the case of the anomalous expansion of ice and bismuth, and a few other substances which at the moment of fusion, and for a few degrees above their point of solidification, exhibit contraction on being heated; but in these instances we have to bear in mind that a change of state is simultaneously effected, or about to be effected, in the substance. Again, certain crystals contract in the direction of one of their axes on the application of heat; but they expand in the direction of another axis, and the total expansion is greater than the contraction, so that they possess a positive coefficient of expansion. Garnets and a few other crystals undergo an increase of specific gravity on being strongly heated, and slowly recover their original density.

The iodide of silver, on the other hand, when far removed from the point at which it undergoes any change of state, appears to exhibit contraction, to possess what M. Fizeau calls a "negative coefficient of expansion;" and this is the more remarkable when we remember that the chlorides, bromides, and iodides of potassinm, sodium, and ammonium, and the chloride and bromide of silver expand considerably when heated, more so, indeed, than the most expansible metals, such as lead, tin, and zinc. The contraction of the iodide of silver is, according to Fizeau, quite regular between $-10°$ C. (14° F.) and $+70°$ C. (158° F.); and he calculates that the contraction is equal to about $\frac{1}{7000}$ of its volume at 0° C. for 100° C., or, again, equal to one sixth

the expansion of platinum for 100° C. He also found that a large hexagonal crystal exhibited a very considerable contraction in the direction of the axis of symmetry, while a slight expansion was produced in a direction normal to the axis of the crystal*. The contraction was observed in the case both of the crystal, a confused crystalline mass, and an amorphous mass produced by strongly compressing the precipitated iodide until it became a hard mass capable of receiving a fine polish, and possessing a specific gravity of 5·569. Fizeau considers that the iodide possesses its maximum of volume or minimum of density at a temperature of $-60°$ C. ($-76°$ F.).

The iodide of silver employed in the following experiments was prepared:—(1) By precipitation. Pure iodide of potassium was added to nitrate of silver, both in dilute solution. The precipitated iodide was thoroughly washed in the dark, slowly dried, fused in a porcelain crucible, and cast into cylindrical masses, either in a warm porcelain or brass mould†. (2) By dissolving pure silver in strong hydriodic acid, evaporating to dryness, fusing. (3) By exposing pure silver leaf for several hours to the vapour of iodine produced by spontaneous evaporation.

Before we examine the effects of heat upon the iodide, it may be well to say a word or two concerning the action of light upon it. A considerable amount of misconception appears to exist in regard to this. Gmelin says "it turns brown on exposure to light, but less quickly than the chloride;" Miller says "it is but slowly acted upon by light;" Fizeau describes it as "noircissant lentement à la lumière;" while Vogel ('Jahresbericht,' 1863) affirms that if it be precipitated with excess of iodide of potassium it is scarcely affected by light, whereas if precipitated with excess of nitrate of silver it changes colour, but undergoes no chemical change. The general idea that it is nearly as sensitive to light as the chloride, has no doubt arisen from the fact that iodides and chlorides are known to have many points of resemblance, and that the iodide is largely used in photography; moreover we remember that a thin film of iodide of silver was the sensitive medium in the original daguerreotype. But we must bear in mind that the change produced by light is not apparent until the so-called "developing solution," which contains reducing agents, has been employed. The change is indeed most obscure: the author of the article on Photography in Watt's 'Dictionary of Chemistry' says of it, "The atoms have apparently acquired a certain degree of mobility, in consequence of which, when submitted to the action of reducing agents, such as ferrous sulphate or pyrogallic acid, they suffer decomposition, the silver being reduced to the

* "Sur la propriété que possède l'iodure d'argent de se contracter par la chaleur et de se dilater par le froid," Comptes Rendus, 1867.

† I must express my indebtedness to Mr. Valentin for allowing me to have a quantity of iodide prepared at South Kensington.

metallic state, and forming an opaque metallic film on the parts of the surface which have been exposed to light."

The following experiments were made to determine the degree of sensitiveness of the iodide to light:—

α. By means of a large lens the rays of the electric lamp were brought to a focus within a glass cell containing a solution of iodide of potassium; a solution of nitrate of silver was then introduced by a pipette at the apex of the cone of rays. The precipitated iodide possessed its usual pale yellow colour.

β. Freshly precipitated iodide in suspension, with a slight excess of iodide of potassium, remained in the full glare of a July sun without undergoing any perceptible change; neither did it subsequently darken.

γ. Freshly precipitated iodide in suspension, with a slight excess of nitrate of silver, underwent no immediate change on exposure to a July sun. At the end of an hour it had become slightly grey, and subsequently darkened.

δ. Organic matter in the shape of starch-paste did not induce any change when mixed with freshly precipitated iodide in suspension with a slight excess of iodide of potassium. Albumenized paper with iodide precipitated upon it did not undergo any immediate change.

ε. Some dried and powdered iodide was found to have acquired a slight greyish metallic tinge after an hour's exposure to the sun. A freshly broken surface of fused iodide became very slightly darker after exposure to the sun. A very pale microscopical crystal of iodide, removed from the interior of a crystalline mass, became slightly brown after several hours' exposure to diffused light.

ζ. Crystals of iodide of silver produced by direct solution of silver in hydriodic acid were not affected by light; neither were crystals of hydro-argentic iodide (AgIHI), nor crystals of argento-potassic iodide (AgIKI).

η. A sheet of silver leaf was exposed to the vapour of iodine (produced by spontaneous evaporation) for five minutes; it possessed a faintly yellow tinge, which on exposure to the sun instantly became pale green, but on further exposure returned to its original pale yellow. A second sheet was exposed for ten minutes to the vapour of iodine; it acquired a golden-yellow surface, which on exposure to diffused light acquired a purplish-red colour, and on exposure to the sun became greenish purple. On continued exposure this colour disappeared, and the plate returned to almost the original yellow colour.

θ. A sheet of silver leaf was exposed to the vapour of iodine for half an hour, at the end of which time it possessed a decided golden yellow colour; on exposure to the sun it instantly acquired a dark purple colour, edged with green at those parts least exposed to the direct vapour of the iodine. On continued exposure the purple became paler, but the sheet did not return to its original yellow colour.

ι. A developing solution composed of ferrous sulphate, alcohol, acetic acid, and water, when applied to the exposed sheets of η and θ, which had been purple, but on continued exposure nearly regained their original colours, produced a reddish-brown colour.

κ. A sheet of silver leaf was exposed to the vapour of iodine for many hours; it was found to be converted into a slightly coherent film of lemon-yellow iodide. Light had no effect upon it, even after long exposure to a July sun; neither was any colour produced on the addition of a developing solution.

The pure iodide of silver would thus appear to be scarcely affected by light, except when silver is present, either in the form of nitrate or, as in the case of the silver films, as metallic silver.

If the precipitated iodide of silver be fused it is found to cool to a greenish-grey mass, which in thin layers is translucent. The surface has sometimes a dark steel-grey, semimetallic appearance, but this does not affect the composition. Sometimes, without any apparent cause, the ordinary greenish surface and the dark steel-grey may exist side by side in the same fused mass. A second fusion may produce a uniformly greenish surface or a uniformly steel-grey surface. But whatever the appearance of the fused mass, it always furnishes when pulverized a lemon-yellow powder, which, when heated, remains unaltered in colour up to about 105° C. (221° F.). At that temperature it begins to darken, and between 105° C. and 180° C. (356° F.) it assumes darker and darker shades of yellow, passing into orange and orange-red; above 180° C. it becomes decidedly red, and darkens through temperatures which may be roughly indicated by the fusing-points of tin, lead, and zinc, until at the latter temperature (412° C., 773° F.) it possesses a very dark brick-red colour. At this temperature the powder becomes coherent, but does not commence to fuse. At a somewhat higher temperature, probably about 450° C. (842° F.), the iodide fuses to a dark-red liquid, the colour of bromine, or of melted sulphur shortly before its boiling-point. At a red heat the iodide begins to volatilize and to decompose, and at a bright red heat this takes place readily. If the iodide be fused and poured into cold water, it becomes a lemon-yellow, amorphous, very brittle mass.

If the fused mass of iodide is allowed to cool, it solidifies to a dark-red transparent body, which is somewhat plastic. On further cooling it becomes much paler in colour, still remaining transparent; and if cooled as a thin film in contact with a hot surface, it passes to a pale yellow transparent variety. The transparent varieties, at a temperature which varies with the mass of the substance, and which in the case of a thin film may be as low as 105° C., become crystalline, opaque, and of a pale greenish-grey colour, somewhat brittle, and of a granular fracture. At the moment of the change from the amorphous, transparent, plastic variety to the opaque, brittle, crystalline variety, considerable expansion

takes place, often accompanied by a loud cracking, and large fissures appear in the mass.

Many attempts were made to determine the precise temperature at which the change from the amorphous to the crystalline condition takes place; but the results were somewhat discordant, depending apparently on the mass of the iodide, and perhaps on the number of times it had been previously fused. The iodide was fused in a glass tube or porcelain crucible, and when fusion was quite complete was placed in an air-bath at 150° C. (302° F.) and allowed to cool. The exact temperature at which the tube was broken by the expanding mass was noticed. About 15 grammes of iodide, which had been often fused, changed suddenly from the amorphous to the crystalline condition at 120° C. (248° F.). Another specimen cracked the tube at 116° C. (240°·8). A porcelain crucible containing 10 grammes of the fused iodide commenced to change at 118° C. (244°·4 F.); the crucible was violently riven open at 105° C. Two small test-tubes, about 6 millimetres diameter and each containing 2 grammes of iodide, were placed in the hot-air bath; the two masses of iodide simultaneously changed to the crystalline condition at 109° C. (228°·2 F.). On one occasion a small mass weighing 3 grammes, prepared by dissolving silver in hydriodic acid, was fused in a tube and slowly cooled. It cooled down to the ordinary temperature of the air without breaking the tube; on moving the tube, however, the mass suddenly underwent molecular change, and the tube was broken. The same iodide fused with some which had been similarly prepared suddenly changed to the crystalline variety at 121° C. (249°·8 F.). From the above results we cannot be far wrong in stating that the change from the amorphous to the crystalline variety of the iodide takes place at a temperature of about 116° C. (240°·8 F.).

Presumably heat is evolved when the amorphous modification of the iodide passes into the crystalline. Several attempts were made to ascertain this by plunging a mass of hot amorphous iodide into hot mercury, inserting a thermometer, and allowing the whole to cool, but no rise of temperature was observed at any given point of the cooling.

If the fused iodide be cast into a tube of porcelain or brass the following effects may be observed:—(α) The mass contracts considerably at the moment of solidification, the level liquid surface sinking into a deep conical depression when it becomes solid. (β) For many seconds after the solidification the solid cylinder of iodide will freely slip out of the tube, and is then seen to be red and transparent, in fact in the amorphous condition; but (γ) if the mass cools until it assumes the crystalline condition it can no longer be got out of the tube; and if the latter be of glass or porcelain, it is infallibly broken by the expansion. Hence if a mass of iodide be allowed to cool in a tube which it cannot break when it expands, it may be made to contract and slip easily out of the tube by heating it. Hence also, as the change from the amorphous

to the crystalline condition takes place at 116° C., it would appear that between the point of fusion, 450° C. (p. 100), and the temperature at which the amorphous iodide becomes crystalline it follows the ordinary law and contracts as it cools, while below that temperature (and, as will be shown, as low as $-18°$ C., $-0°\cdot4$ F.) it expands on getting cooler, and possesses a negative coefficient. It thus appears that when the iodide is in the amorphous condition at 116° C., immediately before the change to the crystalline condition, it is at its point of maximum density.

Several unsuccessful attempts were made to burst metal bottles, after the manner of the familiar ice-experiment, by the expansion of the iodide at the moment when it passes from the amorphous to the crystalline condition. On one occasion, when a somewhat large cylindrical mass had been cast in a tube of thin brass, the latter was burst by the expanding iodide, but thick metal bottles, furnished with a screw, which was forced down into the molten mass, were not broken. Thick porcelain and glass tubes were invariably broken by the expansion; and a good lecture experiment to illustrate the anomalous expansion is furnished by the following means. Let 20 or 30 grammes of fused iodide be cast into a thick cylindrical tube of porcelain a centimetre diameter; in the course of a minute or two the mass has cooled down to the temperature at which it changes from the amorphous to the crystalline condition; it then expands, cracks the tube with a loud noise, and sometimes jerks portions of the tube to a distance of several feet.

A curious effect was noticed in the case of bars of the iodide during cooling. If a bar be cast in a tube and pushed out before it begins to expand, it is seen to curve considerably during cooling. In the case of a bar 15 centimetres long by 6 millimetres diameter, the curvature was such as would be produced with a radius of 48 centimetres, and was always the same with bars of the same length and diameter. A very slight pressure resisted the tendency of the bar to curve. The effect was not due to conduction of heat from one part of the bar while the rest remained perfectly hot; for the effect was the same whether the bar was allowed to cool on a flat copper plate, in an air-bath, or even if it were suspended by a thread of non-conducting matter. It takes place when the iodide passes from the amorphous to the crystalline condition, and is no doubt due to the inequality of strain produced between the outside portions, which first become crystalline and expand, and the internal portions, which assimilate the change less rapidly, for the iodide is a very bad conductor of heat.

It has been stated above that at the moment of solidification of a mass of iodide of silver a considerable contraction takes place. The following experiments were made in order to determine the amount of this contraction. A copper tube, which contained 105·548 grammes of mercury, was found to contain 42·080 grammes of iodide in a molten condition (say 450° C.)—that is, a little above the fusing-point

of the iodide. This would give as the specific gravity of the molten iodide 5·406. The mass was then allowed to solidify in the tube, and a large conical cavity appeared at the moment of congelation. This cavity contained 4·552 grammes of mercury, and would contain 1·8149 gramme of iodide. Hence, if the volume of the iodide before fusion be taken as 100, the volume of the resulting fused iodide will be 104·499. Or, again, 100 volumes of molten iodide contract to 95·694 volumes of the solid. The principal expansion takes place at the moment of fusion, and the expansion between 116° C. and 450° C. is not considerable. No really satisfactory method has been yet found for determining the coefficient of expansion between 116° C. and 450° C.; but if we assume it to be equal to the mean expansion on the other side of 116° C. (of course omitting the sudden expansion which takes place when the amorphous passes into the crystalline condition), we find that a volume 1·00000 at 116° C. will become a volume of 1·01455238 at 450° C. just below the melting-point, while in passing from the solid to the liquid condition the volume increases from 1·01455 to 1·04499, an expansion =·03044.

When a mass of iodide of silver passes from the amorphous into the crystalline condition the molecular commotion is so considerable that portions of the mass are sometimes jerked off from the ends of a bar, and large fissures appear in the mass. These are sometimes as much as half a millimetre broad and several centimetres long in a cylindrical mass weighing from 10 to 20 grammes. They penetrate to the centre of the mass, as may be shown by cooling the iodide under mercury, when the whole mass is found to be permeated by the metal. The capacity of these intercrystalline spaces was determined by allowing a known weight of iodide to pass from its amorphous to its crystalline condition beneath the surface of mercury, and again weighing.

α. 3·643 grammes AgI after thus cooling weighed 3·968 grammes.
β. 5·913 „ „ „ „ 6·417 „

And as we know the specific gravity of mercury and of the iodide, it is easy to deduce from the above that the volume of the cracks is represented respectively by (α) ·1353 gramme and (β) ·2098 gramme of iodide; hence

α.	3·643	:	·1353	: :	100	:	3·7112
β.	5·913	:	·2098	: :	100	:	3·5481

which give a mean of 3·6296. Therefore 100 grammes of iodide in the amorphous condition produce, in passing into the crystalline condition, intercrystalline spaces capable of containing 3·6296 grammes of iodide. From an observation which was made on a cylindrical mass of iodide a centimetre diameter, which in undergoing expansion in the passage from the amorphous to the crystalline condition had produced a separation amounting to half a millimetre in a tube which had yielded to the expansion, the expansion of the mass, *plus* the intercrystalline spaces within

it, was found to be ·047619; hence a volume of amorphous iodide represented by unity becomes a volume of 1·047619 in passing into the crystalline condition, *plus* the intercrystalline spaces; and the volume of these spaces having been determined above, we find that the actual change of volume which takes place simultaneously with the change of molecular condition amounts to ·011323; that is, a volume of iodide at its point of maximum density (116° C.) represented by unity becomes a volume of 1·011323 in changing to the crystalline condition.

Frequent fusion and cooling appear to render a mass of iodide more brittle and crystalline, and to promote the formation of large fissures. The iodide prepared by dissolving silver in hydriodic acid and subsequent fusion was less brittle than that produced by precipitation and frequently fused. We have before noticed that while the latter passes into its crystalline condition at a temperature ranging a few degrees on either side of 116° C., the former may sometimes be cooled to much lower temperatures without change; in fact it appears to be altogether more compact and horny and freer from intercrystalline spaces than the fused iodide produced by precipitation, indeed almost perfectly free. The specific gravity appears also to be slightly higher. Boullay found the specific gravity of the fused iodide produced by precipitation to be 5·61; but he was probably unaware of the presence of the intercrystalline spaces, or he did not take special precautions to obviate them. An ordinary fused mass of iodide was found to have a specific gravity of 5·545, when no precautions were taken to dislodge the air from the intercrystalline spaces. Now we have already given reasons for believing that a volume of iodide at its maximum density (116° C.) becomes a volume of 1·047619 in changing to the crystalline condition; and if we take the specific gravity to be 5·816 at the maximum density, we deduce the specific gravity in the crystalline condition, not taking into account the intercrystalline spaces, to be 5·561,

$$1{\cdot}0476 \;:\; 1 \;::\; 5{\cdot}816 \;:\; 5{\cdot}561,$$

a number which differs by only 0·016 from the specific gravity found by direct weighing, when no precautions were taken to dislodge air from the intercrystalline spaces. When, however, the mass of iodide was boiled for a length of time in water and cooled in a good vacuum, the specific gravity at 0° C. was found to be 5·681. Deville found it under the same conditions to be 5·687*. The specific gravity of the molten iodide has been shown above to be about 5·406, and the specific gravity at the point of maximum density of the amorphous iodide would appear to be 5·8167. This applies to the precipitated iodide which had been several times fused, and with which the principal experiments herein described were made. A specimen of iodide produced by the direct solution of silver in hydriodic acid gave a specific gravity of 5·812, and appeared to be less crystalline

* "Sur les propriétés de l'iodure d'argent," Comptes Rendus, vol. lxiv.

and more compact than that produced by precipitation. Deville found the specific gravity of the unfused precipitated iodide to be 5·807, and of the fused iodide 5·687, while Damour found the native crystals to have a specific gravity of 5·667; hence the amorphous precipitate has a higher density than either the fused crystalline iodide and their native crystals. Thus the density of the amorphous precipitate coincides almost perfectly with the density of the fused iodide at its point of maximum density (116° C.) when in the amorphous condition, as deduced from the above experiments.

If we place in a specific-gravity flask a quantity of fused iodide of silver, fill it up with mercury (taking every precaution to displace the air from the intercrystalline spaces), and place in the ground neck of the flask a perforated stopper continued as a capillary thermometer-stem, we have obviously a thermometer in which we can observe the effect produced by the anomalous contraction of the iodide on the regularly expanding mercury. On applying heat to such an arrangement we observe that for a while the mercury rises in the stem, until on further heating the contraction of the iodide exceeds the expansion of the mercury, and the column retreats if much iodide is present into the very bulb of the instrument.

If the heating be now discontinued the mercury slowly rises as the iodide cools, until the contraction of the mercury exceeds the expansion of the iodide, beyond which point the mercury continues to sink as the bulb cools. Nothing could better illustrate the complete inversion of the effects usually produced by heat on bodies in the case of the iodide of silver.

Professor Guthrie suggested to me that a convenient method of determining the amount of contraction produced by heat in the iodide would be to fill a specific-gravity flask with mercury, and determine the amounts of mercury exuded from the flask for every ten or twenty degrees of temperature; then to place in the flask a known weight of fused iodide with a known weight of mercury, and repeat the determinations. This was accordingly done. About 440 grammes of mercury were employed, and weighings made at intervals of a few degrees. The method was found to be satisfactory, and the results concordant. For example (to take a few instances from many), the amounts of mercury driven from the flask by expansion for 10° C. were found to be as follows:—

Between	29° C.	and	53° C.	·066062
„	22° C.	„	74° C.	·067250
„	20° C.	„	84° C.	·067390
„	29° C.	„	86° C.	·068526
„	47° C.	„	84° C.	·069297

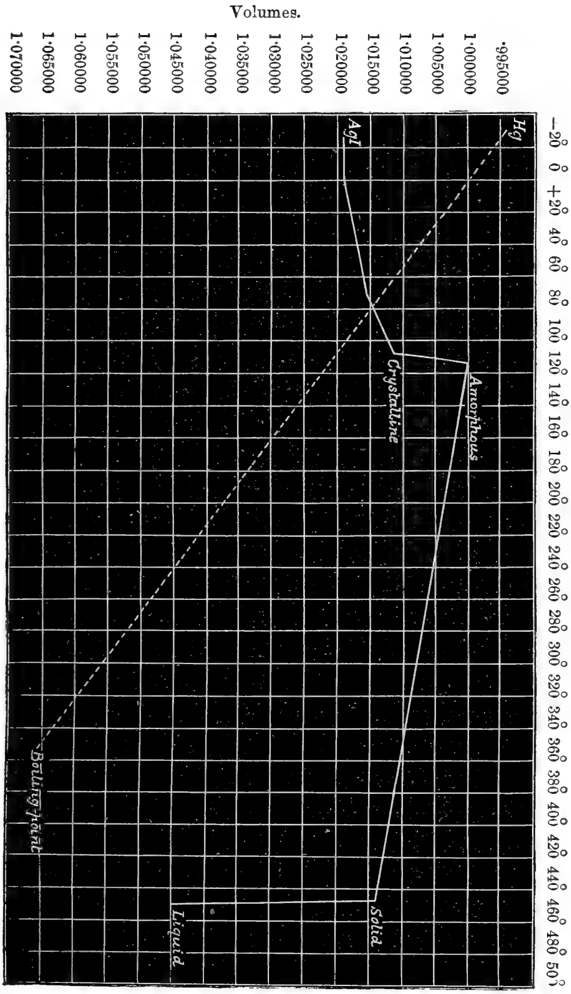
numbers which, when the necessary corrections have been made for

the expansion of the glass, agree very well with Regnault's determinations of the absolute expansion of mercury. Then 38·3680 grammes of the fused iodide were placed in the flask; it was filled up with mercury, the whole was heated, cooled in a good vacuum, weighed, cooled to —18° C. (—0°·4 F.) and weighed. The flask was then heated respectively to 0° C., 21° C., 67° C., and the weights determined. At high temperatures the mercury acts upon the iodide and a green iodide of mercury is formed. The results were not very concordant above 67° C. and have not been introduced. The general results were as follows: the amounts of mercury driven from the flask for 1° C. were respectively—

Between	−18°	and	0° C.	=	·052648
„	−18°	„	+21° C.	=	·051392
„	0°	„	+21° C.	=	·050285
„	−18°	„	+67° C.	=	·049684
„	0°	„	67° C.	=	·048873
„	+21°	„	67° C.	=	·048228

Now from the known weight of mercury in the flask and the known expansion of mercury, it is easy to deduce the quantity of mercury which ought to have been driven from the flask by expansion for any number of degrees; and having determined the actual amount of mercury expelled, we at once find the contraction of the known weight of iodide of silver from known number of degrees by subtracting the amount of mercury expelled from the amount of mercury which ought to have been expelled if the iodide had not been present. We can thus arrive at the coefficient of contraction of the iodide for one degree Centigrade. This appears to be ·00000718 for temperatures between —18° C. and 0° C., ·00003297 for temperatures between 0° C. and 21° C., and ·00005570 for temperatures between 21° and 67° C. Thus the coefficient augments with the temperature. The following Table (p. 107) represents the relationship between the effects of heat on iodide of silver and on mercury. It cannot be regarded as more than an approximation until more perfect methods for investigating the actions can be devised. The extreme peculiarities of the molecular constitution of the iodide, combined with the fact that it cannot be raised to a high temperature in mercury without suffering decomposition, and that its fusing-point is above the boiling-point of mercury, render experiments of this nature, especially when a high temperature is required, unsatisfactory.

TABLE showing approximately the Action of Heat on Iodide of Silver, between −18° C. and the fusing-point, compared with the Action of Heat on Mercury.



I have endeavoured to prove in the foregoing pages the following main facts :—

1. That the iodide of silver exists in three allotropic forms, viz. (α), at temperatures between 116° C. and its fusing-point, as a plastic, tenacious, amorphous substance possessing a reddish colour and transparent to light; (β), at temperatures below 116° C., as a brittle, opaque, greenish-grey, crystalline mass; and (γ), if fused and poured into cold water, as an amorphous, very brittle, yellow, opaque substance.

2. That the iodide possesses a point of maximum density at or about 116° C. at the moment before passing from the amorphous into the crystalline condition.

3. That if we allow a mass of molten iodide to cool, the following effects may be observed :—(a) at the moment of solidification a very considerable contraction takes place; (β) the solid, on further cooling, undergoes slight and regular contraction after the manner of solid bodies in general, until (γ) at or about 116° C. it undergoes sudden and violent expansion, passing from the amorphous into the crystalline condition; (δ) after undergoing this expansion the mass on further cooling undergoes slight expansion, and (ϵ) the coefficient of contraction diminishes as the temperature decreases (or otherwise expressed, the coefficient of contraction augments with the temperature).

I must, in conclusion, express my great indebtedness to Dr. Guthrie for allowing me to carry out many of the foregoing experiments in the Physical Laboratory at South Kensington.

IV. "On the Coefficient of Expansion of a Paraffine of high boiling-point." By G. F. Rodwell, F.R.A.S., F.C.S., Science Master in Marlborough College. Communicated by Prof. Stokes, Sec. R.S. Received November 17, 1874.

In the search for a liquid of high boiling-point which could be used for the exact determination of the coefficient of expansion of iodide of silver, and which at the same time should be free from certain objections which apply to the employment of mercury for this purpose, a paraffine of high boiling-point naturally suggested itself. Mr. W. H. Hatcher, to whom I express my acknowledgments, procured for me a specimen of paraffine which was taken from one of the stills at Messrs. Price's works at a high temperature. With it the following experiments were made.

The paraffine in question is perfectly white and pearly, and in thin layers translucent; it fuses to a colourless liquid, which slightly darkens when heated for some time to a temperature exceeding 600° F. (315°·55 C.). Its specific gravity at 32° F. (0° C.) is ·921. It is hard and somewhat granular when broken, and at a low temperature may be crudely pulverized by a blow. At temperatures exceeding 100° F.

(37°·75 C.) it begins to soften slightly, and gradually becomes softer as the temperature rises, until just before reaching the fusing-point, 142° F. (61°·11 C.), it is quite plastic, and may be moulded by pressure, or spread out into thin coherent sheets. It thus resembles some metals which are brittle at the ordinary temperature, but become more and more malleable as the temperature rises. As the temperature rises, the paraffine becomes more and more translucent; and, like sealing-wax and some other bodies, it becomes what we may call either semifluid or semisolid before it finally fuses to a colourless perfectly transparent fluid. As the temperature approaches 400° F. (205°·5 C.), the liquid begins to give off fumes; it "flashes" (that is to say, the vapour ignites on the application of flame, but does not continue to burn) at 458° F. (236°·65 C.); and the vapour ignites spontaneously without the presence of flame and continues to burn at 576° F. (302°·75 C.). Finally the boiling-point is above the melting-point of lead and the boiling-point of mercury, but below the melting-point of zinc (apparently not much below it), presumably about 750° F. (398°·8 C.). Air-thermometer determinations of the boiling-point were not satisfactory. As the liquid cools from the boiling-point the contraction is seen to be enormous. The comparatively small quantity of liquid paraffine (about 15 grammes) capable of being contained in a tube 15 centimetres (nearly 6 inches) long by 15 millimetres (nearly $\frac{5}{8}$ inch) diameter can be *seen* to sink two or three centimetres in less than 10 minutes. The subsidence continues until the point of solidification is attained, when the mass commences to solidify at the bottom, and proceeds gradually up the sides of the tube, leaving a central core of perfectly fluid matter which does not solidify for some length of time. The mass parts with its heat very slowly. When perfectly and uniformly cooled down to the ordinary temperature, a hollow central core is found in the mass of paraffine, extending nearly to the bottom, and gradually narrowing as it descends. The contraction which takes place in passing from the liquid to the solid condition is very considerable.

The expansion which paraffine undergoes when heated appeared to be so unusnally great, that the following experiments were made in order to determine its amount. They may be divided into three parts, viz. experiments to determine:—(α) the coefficient of expansion of the solid for temperatures between 32° F. (0° C.) and the point of fusion, 142° F. (61°·11 C.); (β) the precise change of volume which ensues when the solid at 142° F. passes into the liquid at 142° F.; and (γ) the coefficient of expansion of the liquid between its point of maximum density (142° F.) and its boiling-point (presumably about 750° F., 398°·8 C.).

In these determinations the greatest obstacle to be contended against was the slight conductivity of the paraffine, which made it very difficult to evenly heat either a solid or liquid mass. The only way of doing this was to keep the mass for a length of time at the precise temperature required; and this it is almost impossible to do when the temperature of

the liquid mass is above the point at which the vapour ignites spontaneously. It is also difficult to manipulate with a liquid which is above the boiling-point of mercury, and is at the same time giving off clouds of spontaneously inflammable vapour. The slight conductivity of the solid is shown by the following results. The bulb of a thermometer was imbedded in a mass of paraffine in such a manner that the distance between the outer surface of the paraffine and the surface of the bulb was 8 millims. (about $\frac{5}{16}$ inch). It was immersed in water, and kept in a fixed position about 3 centimetres above the bottom of the beaker, and on a level with a thermometer-bulb in direct contact with the water. The beaker was gently heated on a sand-bath, and the thermometers were observed for nearly an hour at intervals of two minutes. At starting, 11.20 A.M., the temperature of the water was 52° F. (11°·11 C.). The gas was turned

Time.	Temperature shown by the thermometer immersed in water.	Temperature shown by the thermometer imbedded in paraffine.	Difference.
h m			
11 20 A.M.	52° F.	52° F.	0° F.
11 22	68	54	−14
11 24	85	58	−27
11 26	105	67	−38
11 28	124	79	−45
11 30	141	92	−69
11 32	148	100	−48
11 34	148	107·5	−40·5
11 36	146	112	−34
11 38	144	116·5	−27·5
11 40	141·5	120·5	−21
11 42	139	124	−15
11 44	137	127	−10
11 46	134	129	− 5
11 48	131	130	− 1
11 50	129·5	130	+ 0·5
11 52	128	130	+ 2
11 54	126	129	+ 3
11 56	124	128	+ 4
11 58	122	126	+ 4
12 00	120	124·5	+ 4·5
12 2 P.M.	118	122	+ 4
12 4	116·5	120·5	+ 4
12 6	115·5	119·5	+ 4
12 8	114	118·25	+ 4·25
12 10	112·5	117·25	+ 4·75
12 22	105	111·25	
12 47	90·5	90·5	
1 6	82·5	82·5	
2 14	66·5	66·5	

out when the thermometer immersed in the water read 141° F., but it rose to 148° F. (six degrees above the point of fusion of the paraffine), and a very small amount of paraffine at the surface melted; hence, with the exterior surface actually fusing at 148° F., it will be seen that the imbedded thermometer, separated from the fused surface by only 8 millims. of paraffine, read 48° F. lower. Further, it will be noticed that the temperature of the water rose steadily to 148° F. and then sank at the rate of about one degree in a minute, while the paraffine acquired heat increasingly till the water ceased to be heated, then less quickly, until when the paraffine had acquired a temperature of 130° F. the temperatures coincide, half an hour after the commencement of heating. Then the temperature of the paraffine begins to fall gradually, and less quickly than that of the water, until at 90°·5 F. there is once again coincidence, one hour and twenty-seven minutes from the commencement of heating. After this the temperatures read alike, and the thermometers continue to fall *pari passu*. In the column of differences, *minus* differences signify that the temperatures of the paraffine-thermometer were *below* those of the water-thermometer, *plus* differences that the former were *above* those of the latter.

It will be noticed that the two-minute observations cease at 12.10 P.M., and that the last four are made at intervals of 12, 25, 19, and 68 minutes.

In regard to the fluid paraffine, a mass of from 20 to 30 grammes takes two or three hours to cool down from just below its fusing-point to the temperature of the air—that is, to cool through about 80° F. The bulb of a thermometer was surrounded by 8 millimetres of liquid paraffine at 150° F., and was plunged in a bath kept at 245° F.; at the end of half an hour the mass of about 30 grammes had barely acquired the temperature of the bath. In heating a vessel of paraffine there is always a marked difference between the temperatures of the upper and lower levels. The convection-currents part with their heat so slowly that a uniform temperature throughout the mass seems to be altogether unattainable without constant and complete agitation. During the heating of about half a litre of the liquid paraffine in a cylindrical copper vessel 75 millimetres (about 3 inches) diameter by 150 millimetres high, the following results were obtained:—The upper thermometer was placed with its bulb a centimetre from the surface, the lower thermometer with its bulb a centimetre from the bottom. The fluid mass was directly heated by a Bunsen burner from below. In the column of differences, *minus* differences signify that the temperature of the lower thermometer is *below* that of the upper thermometer, and *plus* differences that the temperatures of the former are *above* those of the latter. The gas was turned out at 6 P.M., the highest temperature being attained by the lower thermometer (361° F.) at 6.1 P.M., while the highest temperature attained by the upper thermometer was 354° F. at the same time.

Time.	Upper thermometer.	Lower thermometer.	Differences.
h m			
5 55 P.M.	270° F.	282° F.	+12° F.
	280	291	+11
	290	298	+ 8
	300	310	+10
	320	331	+11
	330	340	+10
	340	350	+10
6 00	350	359	+ 9
	354	361	+ 7
	354	356	+ 2
	352	348	− 4
	348	337	−11
6 5	336	317	−19
	330	310	−20
	320	298	−22
	310	286	−24
6 10	300	274	−26
	290	264	−26
	280	254	−26
	270	244	−26
6 15	260	235	−25
	250	226	−24
6 20	245	221	−24
	240	217	−23
6 30	198	182	−16
7 20	144	144	0

It will be observed that while observations were made at intervals of 5 minutes (and at first at each of the intervening minutes), the interval between the last two is 50 minutes, and the mass partly solidified had fallen to 144° F. The greatest difference between the upper and lower thermometers during the cooling through a range of 200° F. is no less than 26° F., and only for an instant, at a temperature of about 355° F., have the two thermometers coincided.

Determination of the coefficient of expansion of solid paraffine between 32° F. (0° C.) *and the fusing-point,* 142° F. (61·11 C.).

The above examples show us the extreme difficulty of obtaining uniformity of temperature in either a solid or liquid mass of paraffine, and help to explain various incongruities which presented themselves in the course of the following determinations. The coefficient of expansion of the solid was determined by weighing a piece of compact paraffine in water at different temperatures, the expansion of water (as determined by M. Despretz to seven places of decimals) being of course well known.

Owing to the great contraction which takes place when a mass of liquid paraffine solidifies, it was found to be somewhat difficult to obtain a solid mass free from minute cavities and of uniform texture. The plan eventually adopted was to take a long column of melted paraffine and to cool the lower extremity of it. The supernatant fluid forced its way into the central cavity produced by the contraction of the solidifying mass, and the result was a mass of paraffine apparently quite compact and free from cavities. It was suspended from the balance by horsehair, and weighted by a piece of brass, the weight of which in air and water at different temperatures was known; and the whole was finally weighed in distilled water which had been well boiled and cooled in a good vacuum. After the immersion of the paraffine the whole was again placed in a good vacuum. The mass was heated and weighed in water at various temperatures, between 32° F. (0° C.) and 142° F. (61°·11 C.); the melting-point and the cubical expansion was found to be as follows, for one degree Fahrenheit:—

Between	32° F.	and	60° F. (15°·55 C.)	....	·00031985
,,	60° F.	,,	100° F. (37°·6 C.)	....	·00039090
,,	100° F.	,,	120° F. (48°·85 C.)	....	·00143118
,,	120° F.	,,	142° F. (61°·11 C.)	....	·00244358

The considerable increase of the coefficient as the point of fusion is approached is explained by the fact, already adverted to, that the solid paraffine becomes soft and semisolid like sealing-wax and gutta-percha before actually becoming liquid. From the above coefficients we deduce the fact that

100 volumes	of paraffine	at	32° F.	become	100·8955	at	60° F.
,,	,,	,,	,,	,,	102·4591	,,	100°
,,	,,	,,	,,	,,	105·3215	,,	120°
,,	,,	,,	,,	,,	110·6974	,,	142°

As the latter temperature is approached the mass may be moulded by the hand like a mass of dough or putty; and on continuing the heat at the same temperature, it fuses to a limpid liquid.

Determination of the amount of expansion which paraffine undergoes when it passes from the solid condition at 142° F. *to the liquid condition at the same temperature.*

Tubes of known weight and capacity were exactly filled with melted paraffine at 142°. They were then allowed to cool; the cavity produced by contraction was accurately filled with mercury, which was weighed and the capacity of the cavity deduced therefrom. The results were concordant and satisfactory. Thus it was found that volume=100 at 60° F. becomes, in the fluid condition, at 142° F.:—

I.	115·0565
II.	114·7232
III.	114·9458
IV.	114·7318
	Mean=114·8643

From this we deduce that a volume of paraffine=100 at 32° F. will equal 113·8447 at 142° F. in the fluid state; and if we subtract the expansion between 32° and 142° F. of the solid, we find that the actual expansion of the semisolid paraffine in passing into the perfect liquid will be 3·1473 on the volume 100 at 32° taken as the starting-point.

Determination of the coefficient of expansion of liquid paraffine between 142° F. and its boiling-point (presumably about 750° F.).

The paraffine was heated in tubes of known weight and capacity in a bath of paraffine, and the weight of paraffine which exuded between any observed range of temperature was determined. Specific-gravity flasks with capillary stoppers were found to be unsuitable for the determinations, on account of the difficulty of uniformly heating the mass of liquid within them. Many attempts were made to determine the increase of the coefficient with the temperature, but the results were not satisfactory; hence I can only give the mean coefficient of expansion between the melting-point and the boiling-point.

The result of many experiments gave

·000593

as the mean coefficient of expansion of the liquid. The following are records of some of the determinations:—

A volume of liquid paraffine at 142° F. being taken at 100,

	Found.	Calculated.
Volume at 200° F.	=103·4951	103·4394
,, ,, 250°	=106·4766	106·4044
,, ,, 300°	=108·9820	109·3694
,, ,, 350°	=112·6365	112·3344
,, ,, 400°	=116·4143	115·2994
,, ,, 500°	=122·3814	121·2294
,, ,, 600°	=127·2905	127·1594
,, boiling-point	=135·3344	136·0544

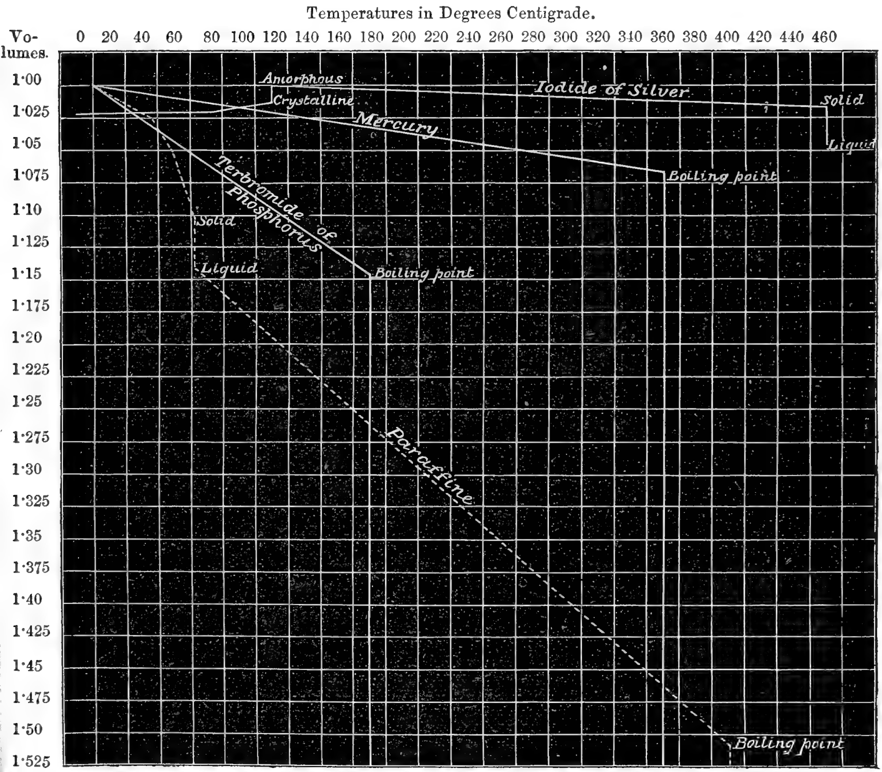
The volume at the boiling-point is calculated on the supposition that it is 750° F., near to which it certainly must be; the volume found corresponds, if we employ the above coefficient of expansion, to 744° F. (395°·5 C.).

Summarizing the above results we obtain the following Table, which represents the volumes of a mass of paraffine (taken at 100 at 32° F.,

0° C.) at various temperatures, and the specific gravities corresponding thereto (sp. gr. at 32° F.=·921).

100 volumes at 32° F. become

100·8955 at	60° F. (15°·55 C.).	Sp. gr.	·913	
102·4591 „	100° F. (37°·6 C.).	„	·899	
105·3215 „	120° F. (48°·85 C.).	„	·874	
110·6974 „	142° F. (61°·11 C.).	„	·884	solid.
113·8447 „	142° F. „	„	·799	liquid.
117·8135 „	200° F. (93°·3 C.).	„	·766	
123·9717 „	300° F. (148°·8 C.).	„	·739	
130·1297 „	400° F. (205°·5 C.).	„	·706	
136·2879 „	500° F. (260° C.).	„	·675	
142·4461 „	600° F. (315°·5 C.).	„	·647	
150 9853 „	744° F. ? (395°·5).	„	·610	



It is thus seen that paraffine is a body which undergoes a most unusual expansion in passing from its ordinary solid condition to the high boiling-

point which it possesses. I do not remember any other substance of a high boiling-point which occupies at the boiling-point a volume which is one half as large again as the volume at the ordinary temperature. In the accompanying figure (p. 115) I have introduced, side by side with the paraffine curve, the expansion curves of mercury, iodide of silver, and terbromide of phosphorus, one of the most expansible liquids known, if we except such bodies as ether, bromide of ethyl, acetate of methyl, &c., the boiling-point of which is below 100° C., and which, therefore, could not easily be introduced into the figure for comparison with a body which boils at nearly 400° C.

V. "Experiments showing the Paramagnetic condition of Arterial Blood, as compared with the Diamagnetic condition of Venous Blood." By Richard C. Shettle, M.D. Communicated by Dr. Lockhart Clarke, F.R.S. Received October 20, 1874.

The magnetic condition of all matter has been well ascertained, and the fact that the same matter may exhibit different magnetic phenomena according to the medium in which it is placed is a point of considerable importance when testing for such results.

It is therefore absolutely essential that any experiments which have for their object the demonstration of paramagnetic force of low power should be tested in media of known strength. In the experiments I have now the honour of laying before the Royal Society, the relative condition which bodies bear to each other as regards their magnetic properties has been strictly observed.

The experiments consist in suspending between the poles of a powerful electromagnet arterial blood hermetically sealed in a glass tube in a medium of venous blood, and venous blood in the same tube, previously well emptied of its contents, in a medium of arterial blood, care being taken to avoid as far as possible any exposure of the blood to the atmosphere, thus preventing any alteration in its physical characteristics as regards the gases which it contains.

The necessary apparatus consists of some German glass tubes in which the fluid to be tested is hermetically sealed, a thin glass vessel for holding the medium in which the testing-tube is suspended, two glass bottles for defibrinating the blood, two store glass bottles for receiving the blood after it has been defibrinated, oxygen gas, carbonic acid gas, very thin india rubber, and an electromagnet and battery of 15 Grove's cells.

The testing-tube (fig. 1) was made of very thin German glass, and the one used for these experiments was of the size and shape shown. It was filled by means of the two short tubes on the upper surface, and when filled was carefully tied over with very thin india rubber; it was suspended by silk in the ordinary way.

The thin glass vessel for holding the medium in which the testing-tube was suspended consisted of an ordinary beaker (fig. 2) of sufficient

Fig. 1.

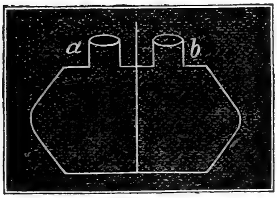
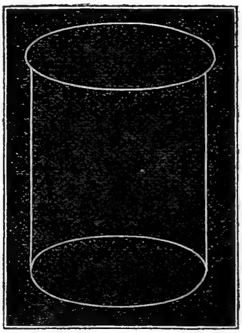
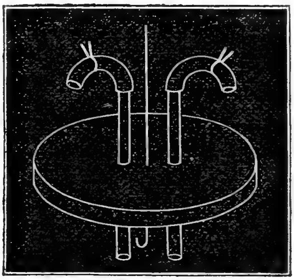


Fig. 2.



internal capacity to allow of the testing-tube rotating freely, without risk of touching the sides. The mouth of this vessel was closed with a cork (fig. 3), the cork being pierced with two glass tubes, to which were

Fig. 3.



attached pieces of india-rubber tubing, capable of being closed when required by means of strong brass clips. This cork was also pierced in its centre by a small copper wire, bent into a crook at its lower end, to which the testing-tube was suspended. The wire was sufficiently long to permit of the testing-tube being raised or lowered as necessary for adjusting the ends to the level of the poles of the magnet.

The bottles used for defibrinating (fig. 4) were wide-mouthed, and capable of holding two pints each; they were well corked, and the corks were pierced with two glass tubes with india-rubber tubing attached, similar to the cork shown in fig. 3. Brass clips were also supplied. Into the under surface of these corks were driven four strips of wood sufficiently long to reach to the bottom of the bottle, and these acted as excellent defibrinating rods.

The store bottles (fig. 5) were narrow-mouthed, capable of holding a pint; they were closed with a cork with one glass tube, fitted with india-

Fig. 4.

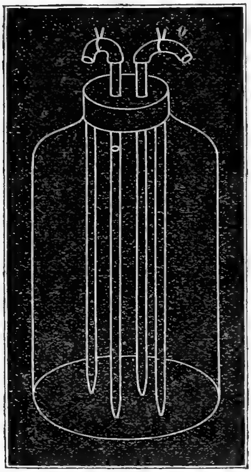
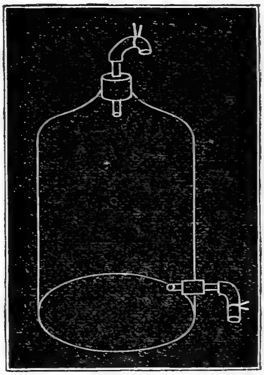


Fig. 5.



rubber tubing and clip as the others. Near the bottom of these bottles was another opening, to which a similar cork with tubing and clip was fitted.

One of these defibrinating bottles and one of the store bottles were filled with oxygen gas, the other defibrinating bottle and the other store bottle with carbonic acid gas; this was done to prevent the blood when drawn from the vessels (artery and vein) being exposed to the atmosphere.

The apparatus being thus prepared, blood was allowed to flow through a glass tube (one end of which was tied into the vein, the other into the india-rubber tube) from the jugular vein of a sheep into the defibrinating bottle filled with carbonic acid gas; both clips having been removed from the india-rubber tubes, the gas flowed out of one tube as the blood entered at the other: during this process, and until defibrination had taken place, the bottle was rotated upon its axis, the clips being re-applied as soon as sufficient blood had been taken. The vein was then properly tied and the carotid artery opened; a similar glass tube was inserted, and blood drawn from it into the defibrinating bottle filled with oxygen gas; the process of defibrination was then performed, as in the case of the venous blood, and the clips again applied.

The liquor sanguinis containing the corpuscles was then drawn off through similar tubes into the store bottles and was ready for testing.

The testing-tube (fig. 1) was readily filled from these store bottles by inserting one of the short tubes (marked *a*) into the india-rubber tubing

attached to the bottom of one of the store bottles, and then removing the clips, which were replaced as soon as the tube (fig. 1) was thoroughly filled: the open tube (*b*) was then speedily tied over with the thin india rubber; the india-rubber tubing connecting it with the store bottle was then carefully removed, and the aperture (*a*) tied over in the same manner; the tube was then carefully wiped and attached by the suspending silk to the crook of the adjusting-wire.

The tube (fig. 1) was first filled with arterial blood, and the vessel (fig. 2) having been filled with carbonic acid gas and placed between the poles of the passive electromagnet, the cork with testing-tube attached, filled as above described, was properly inserted; by means of the adjusting-wire (fig. 3) the points of the tube were made level with the points of the magnetic poles; battery contact was then made, and the tube (fig. 1) took up a diagonal position, pointing north-east by east and south-west by west. The tube connected with the bottom of the store bottle containing venous blood was now connected with one of the glass tubes of the vessel (fig. 2); and the clips being again removed, sufficient venous blood was allowed to run into it to cover the tube (fig. 1), with the exception of the two apertures covered with india rubber, as these marked the position of the tube when immersed in the blood. The tube immediately assumed the direct axial line, then slowly made a half turn and took the axial position again, the ends of the tube being directed to the poles of the magnet, the reverse of that they had first assumed.

Experiment No. 2.

The suspending-tube was now removed and thoroughly emptied of its contents. It was then filled with venous blood in the manner described in Experiment No. 1, and suspended as before, with the exception that the vessel (fig. 2) was this time filled with atmospheric air instead of carbonic acid gas; the tube was then levelled and tested by battery contact being made as in No. 1, and found to occupy an equatorial position. Arterial blood was then allowed to flow into fig. 2 from the store bottle in the same manner as described in Experiment No. 1; the slight oscillations which had previously existed immediately ceased, and the vessel (fig. 1) came to rest in the equatorial line.

Observations.

For the proper performance of these experiments special attention should be given,

1st, to the proper coagulation and removal of the fibrin;

2nd, to the preparation of the blood without contact with the atmosphere;

3rd, to the employment of sufficient battery force, not less than 15 Grove's cells.

1st. It is very necessary that the fibrin be properly removed, for the liquor sanguinis and corpuscles constitute together a medium of con-

siderable density, in which the testing-tube has to rotate; and in calculating the degree of force which exists in the blood, as shown by the rapidity of the movements of the tube under the influence of the magnet, allowance must be made for the resistance from this source that has to be overcome: consequently any fibrin that has not been removed must, in the same degree in which it increases the viscidity and density of the fluid, increase the resistance to the movements of the tube, and so interfere with the manifestation of results.

2nd. By preserving the blood from contact with the atmosphere any change in its physical character from such source is prevented.

3rd. And for the reason that the paramagnetic force in arterial blood must depend upon the amount of oxygen it contains, and the diamagnetic force in venous blood upon carbonic acid, it is evident that the force in the testing-tube as opposed to the force in the suspending medium must be very little, whilst the mechanical resistance afforded by such medium must be considerable. It is therefore essential that the battery-force should be of sufficient power to *develop* these forces to the greatest possible extent.

Addendum.

Since writing the foregoing paper, in repeating the experiments, it has been found that, for the due performance of them, the blood should be maintained as nearly as possible at its natural temperature. To effect this the shape of the vessel (No. 2) has been altered, and it is immersed in a water-bath, the heat of which is sustained by a spirit-lamp, its temperature, and also that of the blood, being regulated by thermometers placed in each vessel.

VI. "On the Multiplication of Definite Integrals." By W. H. L. Russell, F.R.S. Received October 28, 1874.

The definite integral $\int_{y_0}^{y_1}\int_{x_0}^{x_1} \mathrm{P}\,dx\,dy$ may be considered geometrically as the integral $\int \mathrm{P}\,dx\,dy$ extended over an area bounded by the straight lines whose equations are

$$x=y_1, \quad x=y_0, \quad y=x_1, \quad y=x_0.$$

Now conceive the axes transformed through an angle of 45°, so that $x=\frac{\xi}{\sqrt{2}}-\frac{\eta}{\sqrt{2}}$, $y=\frac{\xi}{\sqrt{2}}+\frac{\eta}{\sqrt{2}}$; then the equations to the four straight lines become

$$\frac{\xi}{\sqrt{2}}-\frac{\eta}{\sqrt{2}}=y_1, \quad \frac{\xi}{\sqrt{2}}-\frac{\eta}{\sqrt{2}}=y_0, \quad \frac{\xi}{\sqrt{2}}+\frac{\eta}{\sqrt{2}}=x_1, \quad \frac{\xi}{\sqrt{2}}+\frac{\eta}{\sqrt{2}}=x_0;$$

and computing the integral as extended over an area bounded by the four straight lines thus represented, we have

$$\int_{y_0}^{y_1}\int_{x_0}^{x_1} \mathrm{P}\,dx\,dy = \int_{\frac{x_0+y_0}{\sqrt{2}}}^{\frac{x_0+y_1}{\sqrt{2}}}\int_{y_0\sqrt{2}-\xi}^{\xi-x_0\sqrt{2}} \mathrm{P}\,d\eta\,d\xi + \int_{\frac{x_0+y_1}{\sqrt{2}}}^{\frac{x_1+y_0}{\sqrt{2}}}\int_{y_0\sqrt{2}-\xi}^{y_1\sqrt{2}-\xi} \mathrm{P}\,d\eta\,d\xi$$

$$+\int_{\frac{x_1+y_0}{\sqrt{2}}}^{\frac{x_1+y_1}{\sqrt{2}}}\int_{\xi-x_1\sqrt{2}}^{y_1\sqrt{2}-\xi} \mathrm{P}\,d\eta\,d\xi.$$

After I had discovered this formula, I found that it had been already given in a memoir by Dr. Winckler in the Vienna Transactions for 1862. This memoir treats of the transformation of double integrals between fixed limits, and seems to me one of great interest and importance. My present object is to give two formulæ for the multiplication of definite integrals which will not be found in Dr. Winckler's paper.

$$\int_{y_0}^{y_1}\epsilon^{y^2}dy\int_{x_0}^{x_1}\epsilon^{-x^2}dx = \frac{\epsilon^{-x_0^2}}{2}\int_{y_0}^{y_1}\frac{\epsilon^{z^2}dz}{z+x_0} - \frac{\epsilon^{-x_1^2}}{2}\int_{y_0}^{y_1}\frac{\epsilon^{z^2}dz}{z+x_1}$$

$$-\frac{\epsilon^{y_0^2}}{2}\int_{x_0}^{x_1}\frac{\epsilon^{-z^2}}{z+y_0} + \frac{\epsilon^{y_1^2}}{2}\int_{x_0}^{x_1}\frac{\epsilon^{-z^2}dx}{z+y_1}.$$

Also

$$\int_{y_0}^{y_1}\epsilon^{y^2}dy\int_{x_0}^{x_1}\epsilon^{x^2}dx = \frac{x_1\epsilon^{x_1^2}}{2}\int_{y_0}^{y_1}\frac{dz\,.\,\epsilon^{z^2}}{z^2+x_1^2} - \frac{x_0\epsilon^{x_0^2}}{2}\int_{y_0}^{y_1}\frac{dz\,.\,\epsilon^{z^2}}{z^2+x_0^2}$$

$$+\frac{y_1\epsilon^{y_1^2}}{2}\int_{x_0}^{x_1}\frac{dz\,.\,\epsilon^{z^2}}{z^2+y_1^2} - \frac{y_0\epsilon^{y_0^2}}{2}\int_{x_0}^{x_1}\frac{dz\,.\,\epsilon^{z^2}}{z^2+y_0^2}.$$

The use of these formulæ is easily seen.

December 17, 1874.

JOSEPH DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers and Communications were read:—

I. "On Polishing the Specula of Reflecting Telescopes." By W. Lassell, F.R.S., V.P.R.A.S. Received November 11, 1874.

(Abstract.)

The object of this paper is to describe a method of giving a high lustre and true parabolic curve with ease and certainty, by appropriate machinery, to the surfaces of the specula of large reflecting telescopes.

It may be remembered that many years ago Mr. Lassell invented, and described in the eighteenth volume of the Memoirs of the Royal Astronomical Society, a machine for polishing specula. It is no part of the object of this paper to disparage or supersede that machine, as with it he has polished many specula sensibly perfect, some of which are now in his possession, in whose surfaces he can find no imperfection whatever, and which he should vainly attempt to improve; but it possesses scarcely power enough for polishing a two-foot speculum, though the specula belonging to the telescope of that size which he took out to Malta in 1852 were polished with that machine. Indeed the first surfaces on the four-foot specula of the telescope taken out to Malta on his last expedition were obtained in the same way; but it was with great difficulty, and ultimately the machine broke down hopelessly, the result of which was the construction of the present one described in the paper now presented to the Society. But reference must be had to the paper itself, and to the drawings which accompany it, for an adequate description.

In this machine there may possibly be found nothing very essentially new; it contains parts adopted from others and modified, the principal novelty being a method of giving a regular and gently controlled axial motion to the polisher while it is undergoing the various other motions proper to the machine.

Mr. Lassell attempts in this paper to describe the processes with sufficient precision to enable persons of ordinary intelligence and some mechanical aptitude to obtain with ease and certainty surfaces on specula (taking a two-foot speculum as an example) which shall be sensibly perfect in figure and polish; and this, as his words imply, without the tedious trial-and-error process, which amateurs have had too frequently to experience. Another object he has had also especially in view is to render the process interesting and pleasurable throughout, by devising new modes of performing the most disagreeable parts of the operation, such as the formation of the pitch-tool, which in large surfaces is apt to be very troublesome and annoying. This is accomplished by simply studying the properties of pitch and adapting its treatment, so to speak, to its peculiar unaccommodating humour. A further aim has been to simplify to the utmost the mode of action in every particular, leaving out every thing which long experience has shown to be unnecessary—for instance, polishing with the speculum partly immersed in water, straining pitch through muslin, &c., processes tedious and vexatious enough without being required.

Rules are given in the paper for the rates of motion of both the polisher and speculum, as well as for the lengths of the strokes of the crank-arms, which have an immediate and powerful influence in the production of the required curve. The mode of making the polisher-base, and covering it with pitch in squares approximately to fit the speculum at once, and the mode of keeping the polisher for a considerable time and

through considerable changes of temperature, and renewing the surface for repeated polishings, are also described. The mode of construction of a bed of hones for bringing the curve of the speculum back to the sphere, if it should happen to have gone beyond the parabola in the polishing, without reverting to the emery-grinder, is also explained; and a word or two is added respecting the treatment of the speculum when finished.

II. "Note on the Vertical Distribution of Temperature in the Ocean." By J. Y. Buchanan, Chemist on board H.M.S. 'Challenger.' Communicated by Prof. A. W. Williamson, For. Sec. R.S. Received November 11, 1874.

From newspapers and other reports which have been received by late mails, it appears that the distribution of temperature in the ocean is occupying the attention of a certain portion of the scientific public, and even giving rise to considerable discussion. The observations made on board this ship, and more especially in the Atlantic, have furnished the greater part of the material on which the various speculations have been founded. It appears to me that one point suggested by these observations has not received sufficient attention from those who have written and spoken on the subject: I mean, the effect of the changing seasons on sea-water. Consider the state of the water at and near the surface of the ocean, somewhere not in the tropics. To be more precise, let us suppose that we have taken up our position in the middle of the North Atlantic, somewhere about the 30th parallel. This part of the ocean is not vexed with currents, and affords the best possible field for the observation of the phenomenon in question. The whole ocean enclosed by the 20th and 40th parallels of north latitude and the meridians of 30° and 60° west longitude forms one oceanic lake, not affected by the perturbing influence of currents or of land, and where, therefore, the true effect of differences of atmospheric temperature on the waters of the ocean may be most advantageously studied. Let us assume the winter temperature of the surface-water to be 60° F. and the summer temperature to be 70° F. If we start from midwinter, we find that, as summer approaches, the surface-water must get gradually warmer, and that the temperature of the layers below the surface must decrease at a very rapid rate, until the stratum of winter temperature, or 60° F., is reached; in the language of the isothermal charts, the isothermal line for degrees between 70° F. (if we suppose that we have arrived at midsummer) and 60° F. open out or increase their distance from each other as the depth increases. Let us now consider the conditions after the summer heat has begun to waver. During the whole period of heating, the water, from its increasing temperature, has been always becoming lighter, so that heat communication by convection with the water below has been entirely suspended during the whole period. The heating of

the surface-water has, however, had another effect, besides increasing its volume; it has, by evaporation, rendered it denser than it was before, at the same temperature. Keeping in view this double effect of the summer heat upon the surface-water, let us consider the effect of the winter cold upon it. The superficial water having assumed the atmospheric temperature of, say, 60° F., will sink through the warmer water below it, until it reaches the stratum of water having the same temperature as itself. Arrived here, however, although it has the same temperature as the surrounding water, the two are no longer in equilibrium, for the water, which has come from the surface, has a greater density than that below at the same temperature. It will therefore not be arrested at the stratum of the same temperature, as would have been the case with fresh water; but it will continue to sink, carrying of course its higher temperature with it, and distributing it among the lower layers of colder water. At the end of the winter, therefore, and just before the summer heating recommences, we shall have at the surface a more or less thick stratum of water having a nearly uniform temperature of 60° F., and below this the temperature decreasing at a considerable but less rapid rate than at the termination of the summer heating. If we distinguish between *surface-water*, the temperature of which rises with the atmospheric temperature (following thus, in direction at least, the variation of the seasons), and *subsurface*-water, or the stratum immediately below it, we have for the latter the, at first sight, paradoxical effect of summer cooling and winter heating. The effect of this agency is to diffuse the same heat to a greater depth in the ocean, the greater the yearly range of atmospheric temperature at the surface. This effect is well shown in the chart of isothermals, on a vertical section, between Madeira and a position in lat. 3° 8′ N., long. 14° 49′ W. The isothermal line for 45° F. rises from a depth of 740 fathoms at Madeira to 240 fathoms at the above-mentioned position*. In equatorial regions there is hardly any variation in the surface-temperature of the sea; consequently we find cold water very close to the surface all along the line. On referring to the temperature section between the position lat. 3° 8′ N., long. 14° 49′ W., and St. Paul's rocks, it will be seen that, with a surface-temperature of from 75° F. to 79° F., water at 55° F. is reached at distances of less than 100 fathoms from the surface. Midway between the Azores and Bermuda, with a surface-temperature of 70° F., it is only at a depth of 400 fathoms that we reach water of 55° F.

The above theory of vertical diffusion of temperature in the ocean, owing to convection brought about by the yearly range of temperature at the surface, presupposes that (at least in regions where the range is considerable, and where the great vertical diffusion of heat in question is

* There will, I think, be no violence in assuming an acquaintance with these charts, at least among the scientific public, as they have lately formed the subject of lectures by Dr. Carpenter, and will, no doubt, have been published before this reaches England.

observed) the slightly concentrated water descending from the surface as the winter approaches does not meet water of greater density at the same temperature than its own. Unfortunately the determination of the specific gravity of water below the surface is much less simple than that of the temperature; for although we have an instrument which gives, within any required degree of accuracy, the density of the water at any depth in exactly the same way as the thermometer gives its temperature, the results of the observations are composed of three factors, which depend on the temperature, the pressure, and the *salinity*. By sending down a thermometer along with it we might clear the result for temperature; by noting the depth we might clear for pressure; but the result so cleared would not represent the salinity of the water at the depth in question, but the average excess of salinity of the column of water above it, over or under the mean salinity assumed for sea-water in the calculation of the pressure exercised by a column of it. There remains, therefore, nothing for it but to fetch a sample of water from each depth, and determine its specific gravity on board. As this is an operation which takes up some time, the number of "serial specific-gravity" determinations is comparatively small.

The following are the results of two which were obtained on the voyage between Bermuda and the Azores. The results show the specific gravity at 60° F., that of water at 39°·2 F. being taken as unity.

I. was taken on the 18th June, 1873, in lat. 35° 7′ N., long. 52° 32′ W.

II. was taken on the 24th June, 1873, in lat. 38° 3′ N., long. 39° 19′ W.

For comparison I give one equatorial and one South-Atlantic "serial specific-gravity" determination.

III. was taken on the 21st August, 1873, in lat. 3° 8′ N., long. 14° 49′ W.

IV. was taken on the 3rd October, 1873, in lat. 26° 15′ S., long. 32° 56′ W.

Depth in fathoms.	Specific gravity at 60° F. Distilled water at 39°·2=1.			
	I.	II.	III.	IV.
0	1·02712	1·02684	1·02591	1·02703
50	..	..	1·02658	1·02682
100	..	..	1·02643	1·02649
150	1·02701	1·02677		
200	..	..	1·02620	1·02608
250	1·02683	1·02641		
300	..	..	1·02610	1·02573
400	..	..	1·02629	1·02554
500	1·02604	1·02608		

From the figures in the Table it will be seen that in that part of the ocean the specific gravity of the water in summer decreases from the surface downwards. As a rule it attains an inferior limit at a depth of from 400 to 500 fathoms, which it preserves to the bottom. In those latitudes, therefore, the stratum of intermixture extends down to 500 fathoms; and this may be said also to be the depth to which the sun's influence at the surface penetrates. The results in column III. show the curious phenomenon of the surface-water being specifically lighter than any water below it, and that under an equatorial sun. The position of this sounding was peculiar, inasmuch as it was within line of separation between the Guinea and the equatorial currents. All along the equatorial section the water at 50 and 100 fathoms was found to be specifically heavier than either at the surface or that at greater depths. All along the equator, however, a current runs with great velocity; and I have invariably observed that strong surface-currents introduce considerable irregularities into the specific gravity of the water near the surface. The effect of the greater specific gravity at 100 fathoms conspires, of course within the small yearly range of temperature, in preventing vertical diffusion in the above described manner. Column IV. shows a return in the southern hemisphere to a state of things similar to that which obtains in the North Atlantic.

We have seen that the effect of climate in equatorial regions is to render the subsurface-water much colder than it is in temperate regions; let us consider what would be the effect of a polar climate on the sea-water. It must be observed that the effect of the atmospheric temperature on the sea is determined by the temperature assumed by the surface-water; now the lowest temperature which surface-water can attain is its freezing-point. As the temperature of the air when the 'Challenger' was beyond the 60th parallel was almost constantly below 32° F., freezing must go on to a very great extent in winter; and the effect of freezing such water is, in the end, similar to that of evaporating it; it is separated into lighter ice and denser mother-liquor, which sinks, leaving the ice on the surface. This ice I found to be a mixture; and on determining the melting-point of some in crystals, which had formed in a bucketful of sea-water, I found it began to melt at 29°·5 F., the water produced by it being almost fresh in comparison with sea-water. The lowest temperature of surface-water registered was 27° F.; this happened on two occasions, but was quite exceptional, the usual surface-temperature varying from 32° to 34° F. At this temperature a sensible quantity of ice would melt, giving very light surface-water. On two occasions the specific gravity of the surface-water was found between 1·02400 and 1·02410. The specific gravity increased rapidly up to a depth of 100 fathoms, when it remained pretty uniform to the bottom. Here, as at the equator, it is in winter that the subsurface-water perceives the effect of the change of season, the mother-liquor of the forming ice diffusing in its descent the temperature of its formation.

In the discussion of oceanic phenomena too much attention is usually paid to the great currents. When it is wished to study the phenomena due to temperature, or to any single cause, the effect of the winds, which is seen in its most intense form in the ocean-currents, should be eliminated as far as possible; which in this case can only be done by selecting comparatively motionless seas, like the one which I have mentioned in the North Atlantic*. When the effect of atmospheric climate has been studied on the ocean at large, it would then be proper to apply the experience gained to the consideration of the more complicated phenomena of the currents.

I am at present engaged in a detailed consideration of the temperature and specific gravity results, principally in the direction above indicated, and hope shortly to be able to send it home for publication.

III. "Preliminary Note upon the Brain and Skull of *Amphioxus lanceolatus.*" By T. H. Huxley, Sec. R.S. Received December 17, 1874.

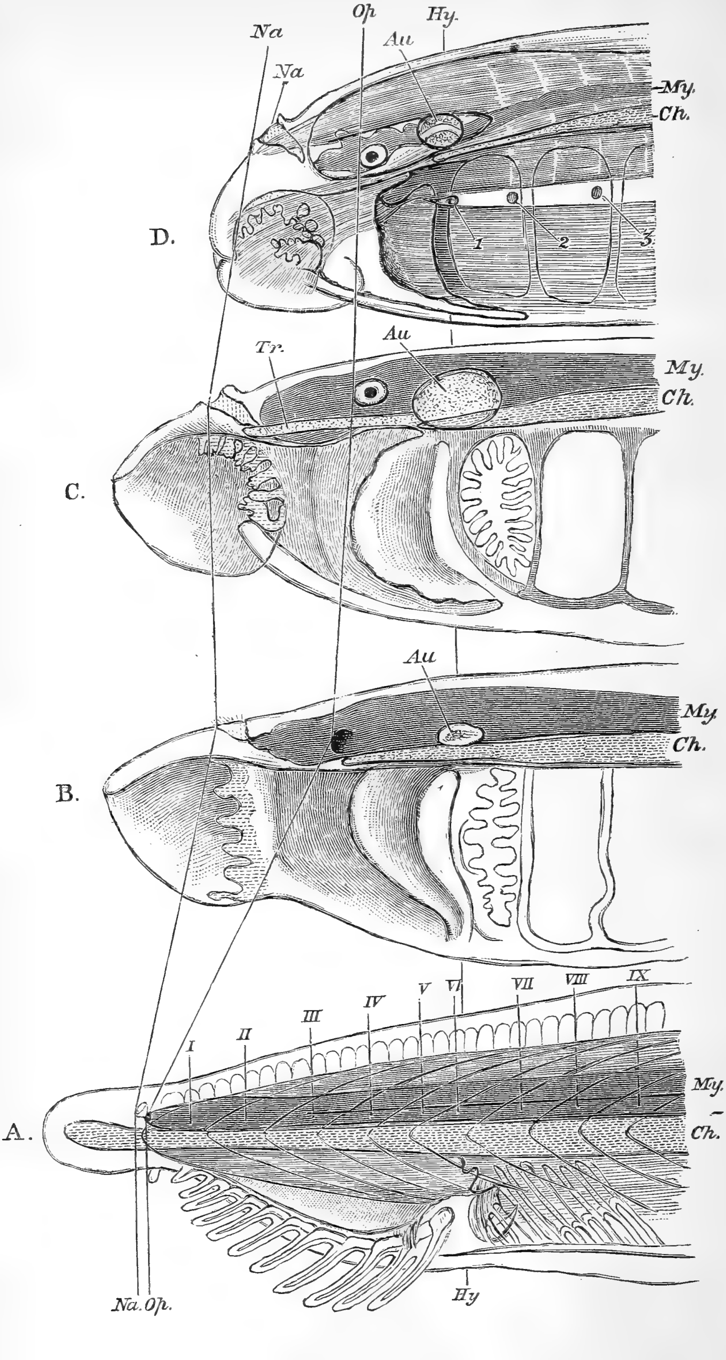
The singular little fish *Amphioxus lanceolatus* has been universally regarded as an extremely anomalous member of the Vertebrate series, by reason of the supposed absence of renal organs and of any proper skull and brain. On these grounds, chiefly, Agassiz proposed to separate it from all other fishes, and Haeckel, going further, made a distinct division of the Vertebrata (*Acrania*) for its reception; while Semper †, in a lately published paper, separates it from the Vertebrata altogether.

In a recent communication to the Linnean Society, I have described what I believe to be the representative of the ducts of the Wolffian bodies, or "primordial kidneys" of the higher Vertebrata, in *Amphioxus*; and I propose, in this preliminary notice, to point out that although *Amphioxus* has no completely differentiated brain or skull, yet it possesses very well-marked and relatively large divisions of the cerebro-spinal nervous axis and of the spinal column, which answer to the encephalon and the cranium of the higher Vertebrata.

The oral aperture of *Amphioxus* is large, of a long oval shape, and

* It will be seen that the principle that the depth to which the effect of the sun's rays penetrates depends on the yearly range of temperature of the water at the surface, explains the presence of the large body of comparatively warm water in the North Atlantic, the existence of which has been usually ascribed to an assumed reflux or back water of the Gulf-stream. The warm water is due to no extraneous cause, but is the natural effect of the conditions of climate at the surface; and the effect of these conditions of climate are so apparent in the temperature of the water, just because it is free from the influence of oceanic currents and exposed to the effects of climate alone.

† "Die Stammverwandtschaft der Wirbelthiere und Wirbellosen," Arbeiten aus dem zool.-zootom. Institut in Würzburg, Bd. ii. 1874, p. 42.


Op
Hy.
Na
Au
Na
My.
Ch.
D.
1
2
3
Tr.
Au
My.
Ch.
C.
Au
My.
Ch.
B.
I
II
III
IV
V
VI
VII
VIII
IX
My.
A.
Ch.
Na. Op.
Hy

fringed by tentacles, external to which lies a lip, which is continuous behind with the ventro-lateral ridge of the body. The oral chamber is spacious, and extends back to the level of the junction between the sixth and seventh myotomes (fig. A). Here it is divided from the branchial cavity by a peculiarly constructed, muscular *velum palati*, the upper attachment of which to the ventral aspect of the sheath of the notochord lies vertically below the anterior angle of the seventh myotome.

Eight pairs of nerves are given off from the cerebro-spinal axis as far as this point. The eighth, or most posterior, of these, which, for convenience, may be called *h*, passes out between the sixth and seventh myotomes, and runs down parallel with the lateral attachment of the velum. The next five (*g*, *f*, *e*, *d*, *c*) pass out between the first six myotomes, and are distributed by their dorsal and ventral branches to those myotomes, to the integument, and to the walls of the buccal cavity. The foremost two nerves (*b* and *a*) pass in front of the first myotome, and the nerve *a* runs parallel with the upperside of the notochord to the end of the snout, giving off branches to that region of the body which lies in front of the mouth. This nerve lies above the eye-spot.

In the Marsipobranch fishes *Myxine* and *Ammocœtes* (now known to be a young condition of *Petromyzon*) a velum also separates the buccal from the branchial cavity (figs. B, C, D). But this velum is in connexion with the hyoidean arch. The resemblance of the buccal cavity, with its tentacles, in *Ammocœtes* to the corresponding cavity in *Amphioxus* is so close, that there can be no doubt that the two are homologous. In the *Ammocœtes* there is a hyoidean cleft which has hitherto been overlooked. The auditory sac lies at the dorsal end of the arch and above the dorsal attachment of the velum. The latter, therefore, corresponds with the auditory region of the skull, and the nerve *h* should answer to the last of the præauditory cranial nerves, which is the *portio dura*. Assuming this to be the case, though the detailed homologies of the cranial nerves of the higher Vertebrata are yet to be worked out, it follows that the segment of the cerebro-spinal axis which in *Amphioxus* lies between the origin of the nerve *h* and the eye, answers to all that part of the brain which lies between the origin of the seventh nerve of *Petromyzon* and the optic nerve. Consequently, the lateral walls of the neural canal in the same region answer to that region of the skull in *Petromyzon* which lies between the origin of the seventh and the origin of the optic nerve. Hence, as each myotome of *Amphioxus* represents the corresponding portion of a protovertebra, it follows that the same region of the skull in the Lamprey and other Vertebrata represents, at fewest, six protovertebræ, almost all traces of which are lost, even in the embryo condition of the higher Vertebrata.

It may further be concluded that the several pairs of nerves which leave the cerebro-spinal axis, between those which answer to the *portio*

dura and the optic nerve, in *Amphioxus*, are represented by the third, fourth, fifth, and sixth pairs of cranial nerves of the higher Vertebrata. The nerve *a*, in fact, has the characteristic course and distribution of the orbito-nasal division of the trigeminal; while, without at present drawing a closer parallel, it is easy to see that the nerves *b*, *c*, *d*, *e*, *f*, and *g*, with their respective myotomes, supply the requisite materials for metamorphosis into the oculomotor, pathetic, trigeminal, and abducens nerves, with the muscles of the eye and of the jaws, in the more differentiated vertebrate types.

Thus, that part of the cerebro-spinal axis of *Amphioxus* which lies in front of the seventh myotome answers to the præauditory part of the brain in the higher Vertebrata, and the corresponding part of the head to the trabecular region of the skull in them. On the other hand, from the seventh myotome backwards, a certain number of segments answer to the postauditory, or parachordal, region of the skull of the higher Vertebrata.

The answer to the question, how many? involves sundry considerations. It must be recollected that though the branchial chamber of *Amphioxus* is the homologue of the branchial chamber of other Vertebrata, it does not necessarily follow that the imperfect branchial skeleton of *Amphioxus* corresponds with their branchial skeleton. The branchial skeleton of the higher Vertebrata consists of cartilaginous rods, which seem to be developed in the somatopleure, and to be homologous with the ribs, while the branchial skeleton of *Amphioxus* consists of fibrous bands apparently developed in the splanchnopleure.

The branchial arches of the higher Vertebrata, in accordance with their essentially costal nature, receive their innervation from the glossopharyngeal and pneumogastric nerves, which are homologues of spinal nerves; and, in seeking for the posterior limits of that region in *Amphioxus* which corresponds with the skull and brain in other Vertebrates, we must only take into account as many pairs of those nerves which arise from the cerebro-spinal axis as we know are, in the Vertebrata next above *Amphioxus*, devoted to the branchial arches. In none of these are there more than seven pairs of branchial arches; so that not more than eight myotomes (and consequently protovertebræ) of *Amphioxus*, in addition to those already mentioned, can be reckoned as the equivalents of the parachordal region of the skull in the higher Vertebrates. Thus it would appear that the cranium of the latter is represented by those segments of the body of *Amphioxus* which lie in front of the fifteenth, counting from before backwards, and that their cranial nerves are represented by the corresponding anterior pairs of nerves in *Amphioxus*.

In all Vertebrata above *Amphioxus* the nerves which answer to the seven posterior pairs in *Amphioxus* unite into one or two trunks on each side, and give rise to the nerves called pneumogastric and glossopharyngeal; and, as these pass out of the skull in front of the occipital

segment, it would appear that this segment is, in the main, the result of the chondrification, with or without subsequent ossification, of the fourteenth protovertebra.

There is no evidence, at present, that the ear-capsule represents a modification of any part of the vertebral skeleton, nor that the trabeculæ are any thing but an anterior pair of visceral arches. And if these parts have nothing to do with centra, or arches, of vertebræ, it follows that the numerous protovertebræ, which lie in front of the fourteenth in *Amphioxus*, are represented only by muscles and nerves in the higher Vertebrata.

The anterior end of the cerebro-spinal axis of *Amphioxus* answers to the *lamina terminalis* of the thalamencephalon of the higher Vertebrata, the cerebral hemispheres and olfactory lobes remaining undeveloped.

If the auditory nerve is, as Gegenbaur has suggested, the dorsal branch of a single nerve which represents both the *portio dura* and the *portio mollis*, the auditory organ of *Amphioxus* is to be sought in connexion with the dorsal branch of its eighth nerve. I have found nothing representing an auditory organ in this position; and I can only conclude that *Amphioxus* really has no auditory apparatus. In all other respects, however, it conforms to the Vertebrate type; and, considering its resemblance to the early stages of *Petromyzon* described by Schulze, I can see no reason for removing it from the class Pisces. But its permanently segmented skull and its many other peculiarities suggest that it should be regarded as the type of a primary division or subclass of the class Pisces, to which the name of *Entomocrania* may be applied, in contrast to the rest, in which the primary segmentation of the skull is lost, and which may be termed *Holocrania*. On a future occasion I propose to show in what manner the skull of the Marsipobranch is related to that of the higher Vertebrata, and more especially to the skull of the Frog in its young tadpole state.

EXPLANATION OF THE FIGURES.

A, C, D are diagrammatic, but accurate, representations of the anterior part of the body in *Amphioxus* (A), in an *Ammocœte* 1·6 inch long (C), and in a fully grown *Ammocœte* 5·7 inches long (D). B is a copy of the furthest advanced stage of the young *Petromyzon planeri* six weeks after hatching, as figured by Schultze in his memoir on the development of that fish. The figures are magnified to the same vertical dimension, so as to afford a means of estimating, roughly, the changes in the proportional growth of the various parts of the head of the Lamprey in its progress from the embryonic towards the adult condition. In C, the brain is already differentiated into the three primary vesicles and the vesicles of the cerebral hemispheres, though they are not shown, the whole brain being merely indicated by the dark shading. The trabeculæ (*Tr*), which have already united in front, are indicated, but not the semilunar ethmoidal cartilage, which lies above and behind the

nasal sac. In D, neither the ethmoidal nor the trabecular cartilages are shown, but the contour of the brain is indicated; and the manner in which the longitudinal muscles, which represent the anterior myotomes of *Amphioxus*, are arranged is shown. The tentacles of *Amphioxus* are represented by the tentacles of the *Ammocœte*, the hood-like "upper lip" of the latter obviously answering to the median prolongation of the head of *Amphioxus* with the two lateral folds of integument which lie outside the bases of the tentacles and are continued back into the ventro-lateral ridges. The relative shortening of the notochord, and lengthening of that region of the brain which lies in front of the origins of the optic nerves, in C, as compared with B, is remarkable.

A line is drawn in all the figures through the anterior margin of the nasal sacs (*Na–Na*); another has the same relation to the eyes (*Op–Op*); and a third (*Hy–Hy*) passes through the region of the auditory sac and hyoidean arch. 1, 2, 3, hyoidean and first and second branchial clefts of *Ammocœtes*; I., II., III., IV., &c., myotomes of *Amphioxus*; *My*, myelon or spinal cord; *Ch*, notochord.

IV. Letters received from the Naturalists attached to the Transit-of-Venus Expedition at Rodriguez.

Government House, Port Mathurin,
Rodriguez, Nov. 2, 1874.

Dear Sir,—I write to give you a short account of my proceedings and success here so far, in my explorations of the Rodriguez bone-caverns.

I must confess to a more or less degree of disappointment on my first inspection of the caverns; and you will understand the cause, I think, when I inform you that out of thirteen caves which I found on my arrival, and which I believed till lately to be the only ones, twelve bore evident, and some recent, signs of previous digging. However, I set to work at once, and, with much diligent search, had found five new caves by the time that we had finished the first thirteen. Out of these I have reason to believe that, in three of them, no mortal foot has ever been previous to mine; for the mouths of all were closed up by a falling-in of the rock, and it was by this sign that I guessed at their existence. We had to work some time at all of them with a big iron mallet before entrance could be effected. In one of these caves I believe I found the bones of two Solitaires, without admixture of those of any other individuals. Of the truth of this I am pretty certain; for they were clearly the bones of a male and female which had fallen down into a cleft, from which egress to so unwieldy a bird was impossible.

Some of the bones had fallen into dust from exposure to the air, being only partially covered with sand, whilst others had been altogether removed, whether by water or not I could not say, for I found no trace of its action there. The same cause, decay, which had nearly annihilated

others might have entirely removed these. I found amongst these about thirty rings of the trachea or tracheæ.

Since then I have found a small hook in another cave, to which it was difficult, from the small size of the entrance, to penetrate. Into this also a slit or cleft from the surface had led, but had since been obliterated. In this I found, I should say, seven "sets" of bones of Solitaire. These were more or less mixed up together by the action of water; but they were still, to a certain extent, in groups, each group being those of an individual. Amongst these I found a perfect skull, with maxillæ attached, and the three parts of the mandible lying close by, four perfect and several injured furcula, and many rings of tracheæ.

I propose soon to try my fortune in a small marsh near here, which looks as if it might originally have been a lakelet or pond. I am induced to do so by the success that my labours met with in a similar locality in Mauritius. I have said "near here," but this is a slip of the pen; I should have said "near my encampment at the caverns."

I have found an immense quantity of tortoise-bones, from which I shall only make a selection before leaving. I have also exhumed a great quantity of bones of smaller birds; but I rather hesitate before giving a description of their genera.

I am afraid that I cannot send any bones by this mail, as the difficulty of transport is so very great. I have every week brought back a few bones of Solitaire, but have had hardly any time to put even these in gelatine, without which operation they would not travel with any degree of safety.

I am, dear Sir,

Very obediently yours,

To Prof. Huxley. HENRY H. SLATER.

Rodriguez, Nov. 3, 1874.

MY DEAR SIR,—A mail being about to leave the island by H.M.S. 'Shearwater,' I now send you some account of my proceedings up to the present time.

I have searched for frogs, more especially tree-frogs, but all the natives of the island tell me that there are none; and as I have neither heard nor seen them, I conclude that this must be the case. With regard to lizards, there is a small house-lizard very abundant. It belongs to the genus *Peripia*, and is very probably the same as that found in Mauritius. It is not only found in houses, but also in trees, beneath the bark of which it lays its eggs. I have been told of a much larger lizard which inhabits a certain part of the island, and have myself searched the spot, but have been unable to find it. I have also offered a reward for a specimen, but have not yet procured one.

The same has been the case with regard to another lizard which lives on Frigate Island, a small island lying off the coast of Rodriguez.

With regard to freshwater fish, there are said to be four kinds, viz.:—

1. A species of perch, commonly called carp here.

2. A species of eel, in most points agreeing with *Anguilla marmorata*, but differing in one important point at least. It undoubtedly enters the streams here, as the specimen which I have was caught about a quarter of a mile above the place whence we get our drinking-water.

3. A species of *Eleotris*, a specimen of which was caught at the same place as the eel. This fish, however, undoubtedly enters brackish water.

4. A species of *Mugilus*. I have my doubts as to whether this fish can really be called a freshwater species.

With regard to the Arachnida, I have collected a considerable quantity of spiders, and have got specimens of the small scorpion which is very fairly common on the island.

A large Scolopendra is very common; but a small species is not so, and I have only succeeded in procuring one specimen.

I have collected a very considerable quantity of insects, more especially of the order Coleoptera.

Peripatus I have not been able to find, though I have made diligent search for it.

There are two species of land-crab, both of which I have procured.

I have only been able to find one very minute species of freshwater sponge, which seems to be very rare. I have only found two small specimens, which, however, came from two streams in widely different parts of the island.

The Vermes are not numerously represented on the island. There are one or two species of *Lumbricus*. There are no leeches in the streams, nor are there planarians either there or on land. There is, however, a beautiful nemertine, which I have found under stones and wood in damp places. I have also found a species of *Gordius* in a stream.

Believe me,

Yours truly,

The Secretary R.S. GEORGE GULLIVER.

P.S.—I do not send any specimens home at present, as, being nearly all in spirit, they still want attention, and it is also necessary for me to keep specimens by me, in order to ascertain whether I have already got specimens which I may find.

Rodriguez, November 1874.

Sir,—I send by the 'Shearwater' to-morrow, for transmission by the mail leaving Mauritius on the 12th instant, a packet of seeds of some of the plants of this island; and, in accordance with my instructions, I submit the following short report of my proceedings here up to the present date.

I have paid special attention to the Palms and *Pandani*. Of the former there are three species indigenous—one of the genus *Latania*, and two belonging to the genus *Areca*. The *Pandani* present much greater difficulty in their determination; and I do not yet feel in a position to fix definitely the number of species, although I rather incline to the idea that there are only two true species. My collection of the plants of the island now numbers about 450 species, of which about three fourths are Phænogams. I have made observations with the view of discriminating between the indigenous and introduced vegetation, but there are several plants regarding which I am doubtful. I have not yet succeeded in finding any marine Phænogams; hitherto, however, I have not devoted much time to the marine flora. There are no tree ferns on the island; at least I have seen none, and, as far as I can learn, none of the inhabitants have seen any. Ferns are represented by about two dozen species; mosses and freshwater algæ are not abundant, but lichens are very numerous, both as species and as individuals. This flora is by no means so extensive as I had expected; but the survey of the island just concluded by the officers of the 'Shearwater' shows the island is only about half the size it was previously supposed to be, it being only 11 miles long by 4 miles broad. The island is a volcanic one, consisting of a succession of lava-flows, radiating from one or more foci in the centre of the island, and now worn away so as to form a series of more or less parallel ridges, separated by deep ravines. These lava-flows are composed chiefly of a dark compact basalt, not unfrequently becoming porphyritic, and commonly exhibiting a marked columnar structure; and I have counted as many as twelve such flows, lying one above the other, separated severally, either by beds of conglomerate, or by beds of laterite, or variously coloured clayey beds. Granite and sandstone do not occur in the island. At the east and at the west ends of the island occur the only non-volcanic rock in the island, namely coralline limestone, extending in huge sheets over many acres of land, and also occurring in detached patches on the top of the basalt, often nearly a mile from the sea. On the northern and southern sides of the island it does not occur; but on the southern side may be seen some raised beaches, marking upheaval there, as does the coralline limestone at the east and west sides. Zeolites are common in the basalt in many places, as also several other minerals. The whole rocks of the island are permeated by iron. This report is very brief;

but I have abstained from entering into details regarding the botany and geology of the island, leaving that for the full report to be given in on my return. I trust, however, the above is sufficient to show that I have made some progress towards accomplishing the objects for which I was sent out here.

I am, Sir,

Yours faithfully,

To the Secretary of the Royal Society. IS. BAYLEY BALFOUR.

The Society then adjourned over the Christmas Recess, to Thursday, January 7, 1875.

Presents received, December 10, 1874.

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The original manuscript Memorial from the President and Council to George III., praying that observers might be sent out to observe the Transit of Venus in 1769. Among the signatures are:—Lord Morton, P.R.S., Nevil Maskelyne, Astron. Royal, Gowin Knight, B. Franklin.

Copy of Diploma on Vellum, with scientific symbols in margin. Engraved for the Society during the Presidency of the Earl of Macclesfield, temp. George II. Presented by Dr. Diamond

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"On some Points connected with the Circulation of the Blood, arrived at from a Study of the Sphygmograph-Trace." By A. H. Garrod, B.A., Fellow of St. John's College, Cambridge, Prosector to the Zoological Society. Communicated by Dr. Garrod, F.R.S. Received March 12, 1874. Read April 23*.

[Plate 5.]

Since my first communication to the Royal Society "On the relative Duration of the Component Parts of the Radial Sphygmograph-Trace in Health" (P. R. S. vol. xviii. p. 351), it has not been my good fortune to find any similar observations by other physiologists, either in favour of or in opposition to my statements. From that time my attention has been continually directed to similar phenomena; and the employment of similar methods has led to results which seem to have an important bearing on the problem of the action of the heart. It is evident that a thorough knowledge of the nature of the pulse in the arteries, when combined with that acquaintance with the anatomical mechanism of the heart and arteries that can be arrived at from *post mortem* examination, is sufficient basis for a fairly thorough study of the circulation of the blood. It has been my endeavour, by the employment of the sphygmograph as constructed by M. Marey, to obtain an amount of information from the curves which it produces, sufficient to generalize on the nature of the cardiac action in some of its details which have not as yet attracted attention. The results will be stated in the form of propositions.

Prop. I. The length of the interval between the commencement of the

* See Proceedings, vol. xxii. p. 291.

ventricular systole at the heart and the closure of the aortic valve does not vary when the pulse-rate is constant, and varies as the square root of the length of the pulse-beat—being found from the equation $xy=20\sqrt{x}$, where x=the pulse-rate, and y=the ratio borne by the above-named part to the whole beat.

This law, in a somewhat modified form, was enunciated by myself in a paper published in the 'Journal of Anatomy and Physiology' (vol. v. p. 17), where the peculiarities of the curves taken in the lying posture misled me as to the point of commencement of the ventricular systole, and led me to state that posture had an effect on the duration of the systole. Such, however, is not the case; for, while lying, the weight of the heart is apparently sufficiently great to neutralize the effect on the trace of the auricular contraction, and to make the thus taken trace deficient in the rise which at other times results from that contraction. At all events if this assumption be made, it is found that the lengths of the different parts of the beat are not influenced by posture, and they agree exactly with the above-stated law.

The following are measurements made since the publication of the original paper, which tend fully to confirm the above statement:—

Pulse-rate.	Number of times the first part is contained in the whole beat.	Calculated ratio of first part to whole beat on formula $xy=20\sqrt{x}$.
46	2·925	2·93
48	2·8, 2·88	2·885
49	2·85	2·86
52·5	2·71	2·765
56	2·63	2·675
57	2·75	2·66
58	2·65	2·625
60	2·63	2·59
64·5	2·556	2·49
69	2·45	2·4
74	2·28	2·325
79	2·23, 2·275	2·24
80	2·24375, 2·207	2·225
81·5	2·2, 2·185, 2·093	2·2
84	2·105	2·175
85	2·09	2·16
86	2·17, 2·053	2·155
88·5	2·245, 2·275	2·11
90·5	2·062	2·1
92	2·12	2·09
92	2·0875	2·08
94	2·14125	2·05

Prop. II. The length of the interval between the commencement of the primary and the dicrotic rises in the radial artery is constant for any given pulse-rate, and varies as the cube root of the length of the pulse-beat—being found from the equation $xy'=47\sqrt[3]{x}$, where x=the pulse-rate, and y'=the ratio borne by the above-named part to the whole beat.

This law was enunciated in the paper before referred to as read before this Society by myself, and published in its 'Proceedings' (vol. xviii. p. 351). Since that paper was read a fresh series of measurements have strongly confirmed its accuracy, and practice in manipulation has diminished my limits of experimental error so far that a difference of 5 per cent. from the calculated results is rarely found.

The following Table contains some of the more recent results and one or two more careful measurements of old traces :—

Pulse-rate.	Ratio borne by sphygmosystole to whole beat, as	
	Found by measurement.	Calculated from equation $xy'=47\sqrt[3]{x}$ (approximately).
38	4·175	4·18
43·5	3·825	3·8
44·5	3·7875	3·75
56	3·29	3·22
58	3·195	3·135
59	3·185	3·0·
63	2·911	2·96
63·5	2·938	2·95
64	2·904	2·93
65	2·83, 2·821	2·9
67	2·825, 2·788	2·84
66·5	2·889	2·795
69	2·7	2·78
73	2·625	2·685
105	2·132	2·13
140	1·735	1·75

Prop. III. The length of the interval between the primary and the dicrotic rises follows the same law in the carotid and posterior tibial that it does in the radial artery.

That such is the case as far as the femoral and posterior tibial arteries are concerned is shown by Dr. Galabin in a paper "On the Secondary Waves in the Pulse," recently published (Journal of Anat. and Phys. 2nd series, No. xiii.). It is not necessary, in proving this law, to undertake any large series of measurements; for if *all* those which are taken agree exactly with the calculated results obtained from the radial equation, the probability that it is correct is almost infinitely great. In a boy, ætat. 16, whose radial pulse was previously proved to

follow the above law exactly, the following are the results obtained by measuring the carotid tracings :—

Pulse-rate.	Ratio borne by sphygmosystole to whole beat, as	
	Found by measurement.	Calculated from radial equation.
67	2·899	2·84
68	2·827, 2·6	2·8
72	2·7144	2·7
77	2·583	2·59
77	2·594	2·59

In another subject, ætat. 22, the following are the results :—

77	2·6625	2·595
78	2·575	2·575
85	2·443	2·44

With regard to the posterior tibial artery, most of the results were obtained by the employment of the double sphygmograph, to be described further on, in which the superposition of the simultaneous posterior tibial trace on that from the radial artery showed that the interval between the commencing primary and dicrotic rises is the same in both. The following are a few independent measurements from tracings from the artery behind the ankle :—

Pulse-rate.	Proportion borne by first part to whole beat in ankle trace.	Proportion borne by first part to whole beat in radial trace (approximately).
70	2·7	2·76
73	2·675	2·685
80	2·596	2·525
82	2·4575	2·5
82·5	2·517	2·495
88	2·378	2·378

Corollary.—The length of the interval between the primary and secondary rises being exactly the same in the carotid, radial, and posterior tibial arteries, which are three vessels at very different distances from the heart, it is evident that *the length of this interval is constant throughout the larger arteries, and must be of the same duration at the origin of the aorta that it is in the radial artery at the wrist.*

The corollary to Proposition III. leads to theoretical results of considerable importance; for as the duration of the different elements of each beat in the radial artery is the same as that in the commencing aorta, by superposing the sphygmograph-trace upon the cardiograph-trace at any given pulse-rate, a comparison can be made between the duration of the different physiological changes going on in the heart and those going on in the commencing aorta; in other words, the time during which the ventricular and arterial systoles are continuous can be ascertained with precision by an indirect method, which alone is possible in the human subject.

Taking the equations given in Prop. I. and Prop. II., the length of the systolic portion of each beat in the cardiograph- and sphygmograph-traces may be calculated with facility for any value of x. From the equations above given, namely, $xy=20\sqrt{x}$ and $xy'=47\sqrt[3]{x}$, it is found that the length of the arterial systole is shorter than the cardiac, as would be expected, because the cardiograph-trace is an indication of the movements in the muscular walls of the heart, and not of the contained blood, and because a certain tension must be reached by the intraventricular blood at the commencement of the systole before it can push open the aortic valves.

The sphygmosystole being therefore shorter than the cardiac systole, it becomes a question, when an attempt is made to superpose them exactly, as to whether they correspond at the commencement or the end of the cardiosystole. This is easily answered; for independent observations show the points in both at which the semilunar valves of the aorta close. These points in the traces must evidently be simultaneous, which is therefore the same thing as saying that the interval between the greater cardiosystole and the shorter sphygmosystole is at the commencement of the cardiosystole. This interval, the existence of which is well indicated in Marey's cardio-aortic tracings from the horse, may be termed the *syspasis* (the time during which the ventricles are raising the pressure of their contained blood); and the following Table, obtained from the two equations just mentioned, gives its length at different pulse-rates:—

·0018753′	at	$x=36$	approximately.
·00132986′	,,	$x=49$	,,
·000931′	,,	$x=64$	,,
·00004199′	,,	$x=81$	,,
·0003766′	,,	$x=100$	,,
·00024645′	,,	$x=121$	,,
·000118′	,,	$x=144$	,,
·000000′	,,	$x=170$	,,

From this Table it is evident that the syspasis varies considerably with different rapidities of pulse, decreasing rapidly with increase in the

pulse-rate and becoming *nil* when it is 170 a minute, which may be fairly conceived to be very near the limit of cardiac rapidity in man.

That this interval (the syspasis) should vary so considerably in length with differeut pulse-rates is not easy to explain at first sight; nevertheless a careful review of the different processes which are in operation in the heart at the time has suggested to me an explanation which seems reasonable. It depends on the fact that the extreme shortness of the diastole makes any variation in its length have a marked influence on the amount of blood which enters the capillaries of the walls of the heart, and consequently influences the amount of work which the muscular fibres of the ventricle have to perform in emptying their interstitial vessels before they can commence contracting on the blood in their contained cavity. Experiment shows that the rapidity of the pulse does not depend on the pressure of the blood in the arterial system*; consequently the length of the syspasis is not influenced by the arterial blood-pressure, which is the same thing as saying that the force of the cardiac contraction varies directly as the blood-pressure; for then the muscular power of the ventricular walls to overcome the intramural distention, varying with it, prevents its duration from being modified.

It has been my endeavour to show elsewhere† that the force of the heart's contraction is modified by the length of diastole, varying as its square root. Such being the case, it is evident that the length of the syspasis must vary with that of the diastole, though not to the extent that is found to occur. But the diastolic period being always so short, it is evident that the longer it is, the more thoroughly does the heart-tissue get permeated with blood, in a way which can have little or no influence on its nutritive power, but a great effect in modifying the length of the syspasis in the direction which is found to occur.

Again, referring to the results of the cardio-sphygmograph observations published by me in the 'Proceedings' of this Society (vol. xix. p. 318), that paper contains a Table of the length of the different cardio-arterial intervals; and if from the first cardio-arterial interval, as there defined, the length of the syspasis be subtracted at the corresponding rates, it will be found that the remainder of the interval is of exactly the same length as the second cardio-arterial interval, which, on the assumptions made, it could only be, as both the systole and the shock of the closure of the aortic valve are propagated along the arteries from the same point under similar circumstances. The following Table gives the lengths of the first cardio-arterial interval from which that of the syspasis as above determined has been subtracted, and by their side the lengths of the second cardio-arterial interval, as copied from the Table in the communication referred to; *their similarity cannot be the result of simple coincidence*, as they are derived from independent sets of measurements.

* Journal of Anatomy and Physiology, Nov. 1873.

† *Ibid.* vol. viii.

Pulse-rate.	First cardio-arterial interval with syspasis subtracted.	Second cardio-arterial interval.
36	·0023982′	·00239821′
49	·00233314′	·00233342′
64	·002274′	·00227425′
81	·00220541′	·00220546′
100	·0021875′	·00218745′
121	·00208455′	·0020847′
144	·0020185′	·0020185′
170	·0019704′	·0019729′

After the completion of the cardio-sphygmograph tracing above referred to, it was my endeavour to obtain satisfactory double sphygmograph tracings from arteries at different distances from the heart. Two or three unsuccessful attempts suggested the plan which has proved successful. It was soon evident that there is only one artery, other than the radial, which it is possible to manipulate with any degree of facility, especially when the experimenter is the subject of experiment. This artery is the posterior tibial at the ankle, where it runs in the interval between the internal malleolus and the tuberosity of the os calcis, just before it gives off the internal calcaneal branches. On myself, this artery is as superficial and as easily reached as the radial; in the sitting posture it is quite under command when the foot is crossed over the opposite knee; it is considerably further from the heart than the radial; and to obtain as great a difference as possible, the right wrist was on all occasions the one experimented on, the wrist and the ankle being, as far as can be estimated on the living body, 29 inches and $52\frac{1}{2}$ inches respectively from the aortic valves.

Before going further it will be necessary to consider *the sphygmograph-trace from the posterior tibial artery at the ankle.* Wolff * has published the results of his observations on the *dorsalis pedis* artery; and as they correspond with those from the ankle trace of the posterior tibial, they may be recapitulated. He remarks that the pulse at the foot has a general resemblance to that at the wrist, it differing in the primary ascent being less abrupt and the summit less acute. In the descent the secondary undulation is remarkably insignificant. The other minor undulations are less constant. My observations confirm the above with respect to the general similarity between the two pulses, the greater obliquity of the primary rise, and the less constant character of the minor undulations; the secondary rise has, however, never struck me as peculiarly insignificant, though it has peculiarities, to be mentioned immediately.

The ankle trace of a pulse at about 70 a minute, as taken with an

* Characteristik des Arterienpuls.

ordinary sphygmograph, differs from that at the wrist in more than one point. The primary rise, as previously mentioned, is less abrupt; the following fall is more considerable, and is not broken by the notch nearly constantly seen in wrist traces of this rapidity. The secondary rise starts from a lower level and is well marked, reaching its climax considerably nearer the next primary rise than in the wrist trace. There is, however, another feature in the early part of the secondary rise in the ankle trace, which deserves special attention because of its general occurrence. As is well known, in wrist traces the secondary rise commences promptly and is quite uniform in character, but in ankle traces there is nearly always a short horizontal continuation of the curve immediately following the primary fall, the point of departure of the two lines being clearly indicated by an abrupt, though not considerable, change in direction. This horizontal portion of the trace is not of any considerable length, being in a pulse of 70 a minute about one eighth of the whole beat; it is followed by a well-defined secondary rise, which is much longer and more gradual than the primary. Though described above as horizontal, this short interval between the two undulations is not so always, being frequently slightly oblique, sometimes in one direction, sometimes in the other. When its curve is downwards (that is, when it tends in the same direction as the primary fall), it may appear to be part of that event, which would then look as if broken; when its curve is upwards (that is, when it tends in the same direction as the secondary rise), it makes the trace appear more normal in comparison with that from the wrist.

Having now explained the ankle sphygmograph-trace, in considering the simultaneous wrist and ankle traces it will be necessary to commence with the description of the instrument employed to obtain them. A drawing of it from above is seen in Plate 5. fig. 1, from the side in fig. 2, and a double sphygmogram is given in fig. 4.

The double sphymograph is constructed from two of the ordinary sphygmographs of Marey, as first constructed by Breguet. One, that employed in taking the ankle trace, retains all its original parts, except the side lappets for fixing it to the arm, and its recording-apparatus receives the double trace. A second lever is fixed in connexion with it by two uprights so placed as to allow the axis of the second lever to be parallel to and above the one belonging to the instrument, sufficient room being left to allow the latter to move unobstructed up to the top of the recording paper. This second lever, which is a *facsimile* of that used in the sphygmograph, is placed so that it will write on the same recording-paper as the first; but its position is reversed. The accompanying sketch (fig. 3) will show this point, it representing a side view of the ordinary knife-edge lever upside down—that is, with the surface (*s*) on which the knife-edge ought to slide uppermost. The object of this arrangement will be seen immediately.

The second sphygmograph has the watchwork removed, as well as the

brasswork which is fastened to the spring that presses on the pulse, to the end of which a small wire loop is soldered. In addition, a small piece of wood is screwed into the nearer of the two holes by which the watch-work was fixed, in such a way that it can be made to revolve with difficulty. The two instruments are fastened together by means of a screw and nut in the foot-sphygmograph, which bind a brass plate in that for the wrist. This screw is fixed on a plate of brass which is attached to the end of the instrument furthest from the watchwork in the manner shown in the figure. The brass plate in the other sphygmograph, which it binds, is fixed on the side of the body of the instrument close to the arbor of the lever. The exact position of these additional pieces of brass-work has to be determined by the direction that a silk cord takes when, fixed at one end to the arbor-end of the inverted lever mentioned above, it is threaded through the loop on the tip of the spring of the wrist-sphygmograph. This cord has to be parallel to the sides of the ankle-sphygmograph, when the two instruments are fastened together with the nut at right angles to one another.

On commencing to take a double trace, the nut is unscrewed, and the two instruments are separated from one another. The wrist-sphygmograph is then bound, as usual, on the right arm. The silk cord attached to the arbor-end of the wrist-pulse lever (the upper one in the ankle-sphygmograph) is then threaded through the loop at the tip of the wrist-spring, and the binding-screw to fix the two instruments is passed into the hole in the plate of the wrist-sphygmograph made to receive it; after which, the nut being screwed fast down, the two sphygmographs form a single mass. The silk cord is then carried round the piece of wood at the watchwork end of the wrist-sphygmograph, and, after being slightly tightened, is fixed in a groove on its side. The whole is now ready for commencing the trace. To do this the ankle instrument (with that for the wrist attached to it and to the arm) is placed over the left foot, which has to rest on the right knee, parallel to the direction of the leg, with the watchwork towards the body. The recording-paper is placed in position; the silk thread is tightened, by slightly turning the wooden peg to which it is fixed, and the wrist-lever is made to pulsate by it towards the upper part of the recording-paper. The ankle-sphygmograph, held by its watchwork end in the left hand, and attached at the other extremity to the right wrist, is then pressed down on the inside of the left foot (which rests on the right knee), in such a way that its pulse-pad compresses the posterior tibial artery where its pulsation is most manifest. The lever is made to record on the lower part of the smoked paper, below the one connected with the wrist. When both levers are found to be working freely, the recording-paper is set moving by liberating the watchwork-catch with the left thumb, which is close to it. The respiration must be checked during the time the recording-paper is moving, to prevent irregularities in the trace.

Results arrived at from the study of the simultaneous wrist and ankle tracings.

In employing the tracings obtained from the above compound instrument, two objects were kept in view—*first*, to find the interval between the commencement of the primary rises in the wrist and ankle curves; and *secondly*, to observe whether or no the superposition of the one trace upon the other verified or falsified the statement made in Prop. III., that the lengths of the different parts of each element of the curve were the same in the two arteries.

The following Table contains the measurements of the lengths of the intervals between the commencement of the primary rise in the wrist and ankle tracings at different rapidities of pulse, from which it is clear that this interval varies very slightly within the range that can be obtained, and that the tendency is for it to be very slightly longer in the slower pulses.

Rapidity of pulse.	Length of interval between commencement of systolic rise at the wrist and at the ankle.
62	·00115′ occurring 14·08 times in each beat.
63	·00125′ " 12·7 " " "
67	·001343′ " 11·11 " " "
"	·0013278′ " 11·24 " " "
70	·001222′ " 11·7 " " "
71	·00136′ " 10·2 " " "
"	·00124′ " 11·41 " " "
"	·0013′ " 10·8 " " "
72	·0012′ " 11·7 " " "
"	·001206′ " 11·52 " " "
79	·001145′ " 11·06 " " "
80	·00126′ " 9·96 " " "
81	·001233′ " 10·37 " " "
82	·001123′ " 10·86 " " "
"	·00122′ " 10 " " "
95	·00122′ " 8·67 " " "
98	·001085′ " 9·7 " " "
99	·00116′ " 8·607 " " "

which gives an average length of ·0012314 of a minute for all the rates.

It being possible to estimate with considerable accuracy the distance from the aortic valves of the spots on the arteries at which the instrument is usually applied, it becomes a point of interest to determine from the facts arrived at the rapidity with which the primary undulation travels from its origin (the heart) to the peripheral vessels. The radial artery at the wrist and the posterior tibial artery at the ankle are, as nearly as can be determined, 29 inches and 52½ inches respectively from the origin of the aorta in myself (on whom all the tracings have been taken), as previously mentioned; and as the time of transit of the wave

varies very little with different rapidities of pulse, a single example may be taken to illustrate the point in question. With the heart beating 100 times in a minute, the time taken by the primary wave in reaching the wrist (that is, the length of the first cardio-radial interval with the syspasis subtracted) has been shown in a previous Table to be ·0021875 of a minute. Adding to this the interval between the radial and ankle primary rise at the same rapidity, which is very nearly ·00116 of a minute, ·0033475 of a minute is the time taken by the systolic wave in travelling from the heart to the ankle. But if this wave went the extra distance to the ankle, (52·5—29=) 23·5 inches, at the same rate at which it reaches the wrist, the length of the first cardio-malleolar interval would be ·00459375 of a minute (29 : 52·5 : : 21875 : 459375); but it is only ·0033475 of a minute, which is considerably less; consequently *the wave augments in rapidity as it gets further from the heart*, a phenomenon beyond my power to explain.

By superimposing the wrist trace from a simultaneous sphygmogram on that from the ankle, it is found that the components of each are of exactly similar duration, though the peculiar short interval following the dicrotic notch in the latter sometimes complicates the results. This exact similarity in length of the different elements of the two pulses is not, as will be found by those who attempt to measure them practically, self-evident from the tracings themselves; because the one being slightly later than the other, and the watchwork varying in rapidity, gradually increasing and then declining, the radial, which is the earlier, is slightly the shorter in the commencement of the trace and the longer towards its end. In the middle of the recording-paper the two coincide. It may therefore be said that the compound sphygmograph-trace is entirely in favour of the correctness of Prop. III.

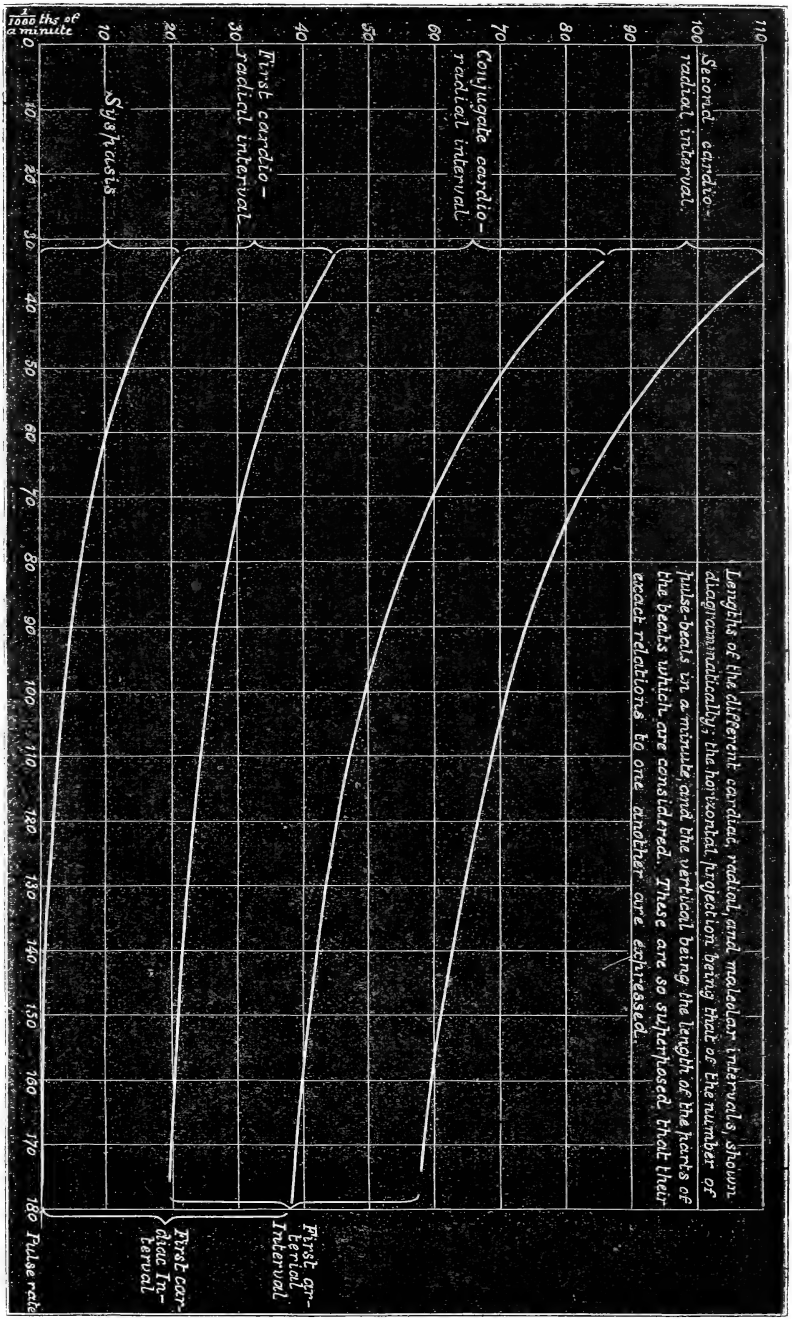
In conclusion, the following is a summary of the results arrived at in this communication:—

I. The lengths of the different elements of the pulse-beat being the same in arteries at different distances from the heart, the radial sphygmograph-trace expresses their duration in the aorta.

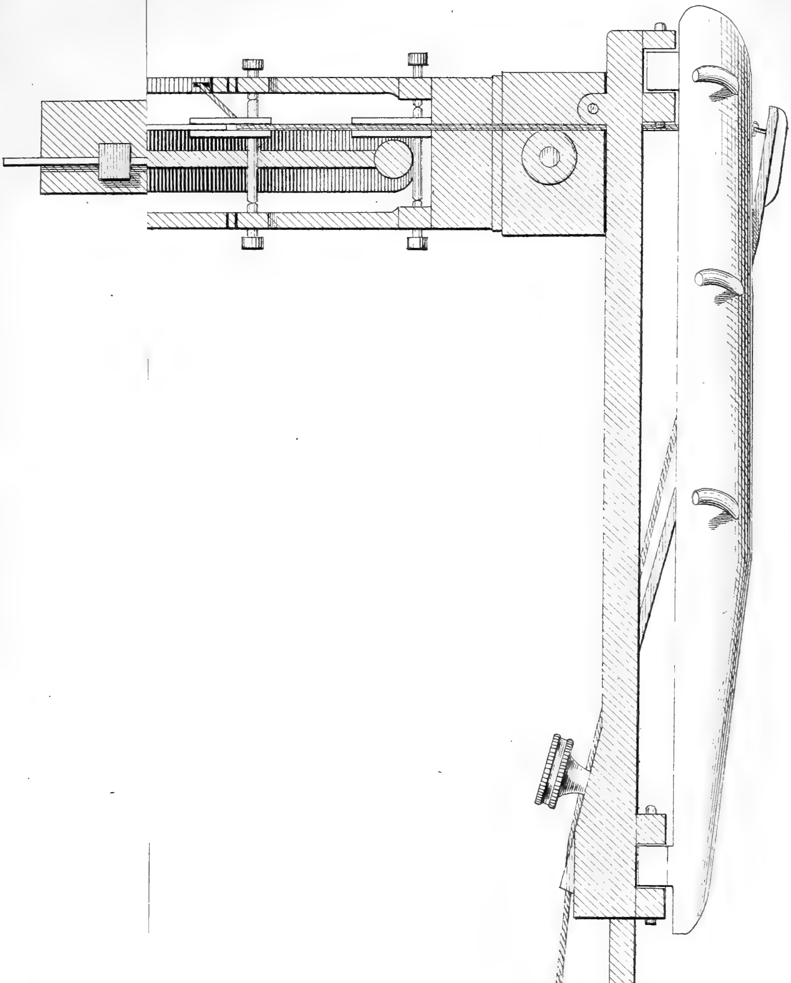
II. The cardiosystole being longer than the sphygmosystole at all possible pulse-rates, the excess in the length of the former expresses the time required by the heart to reach, from a state of rest, a systolic pressure sufficient to open the semilunar valves. This interval, termed the *syspasis*, is constant for any given rapidity of cardiac action, and rapidly decreases as the pulse gets quicker, becoming *nil* at a rate of 170 a minute.

III. The interval between the commencement of the primary pulse-rise in the radial and that in the posterior tibial artery is less than would be estimated from the time taken by the same wave in travelling from the aortic valve to the radial artery.

The woodcut (p. 151) will assist in illustrating the mutual relations of the different component parts of the cardiac revolution, as its different elements are there shown in their actual relations one to the other.



1/1080 ths of a minute
0 10 20 30 40 50 60 70 80 90 100 110 120 130 140 150 160 170 180 Pulse rate
10 20 30 40 50 60 70 80 90 100 110
Second cardio-radial interval.
Conjugate cardio-radial interval
First cardio-radial interval
Systasis
First arterial Interval
First cardiac Interval
Lengths of the different cardiac, radial, and malleolar intervals, shown diagrammatically; the horizontal projection being that of the number of pulse-beats in a minute, and the vertical being the length of the parts of the beats which are considered. These are so superposed that their exact relations to one another are expressed.



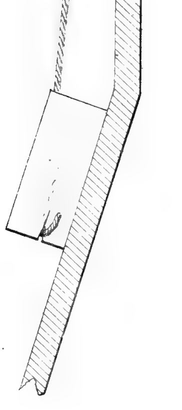
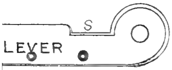


Fig. 3.



W. West & C° imp.

Fig. 1.

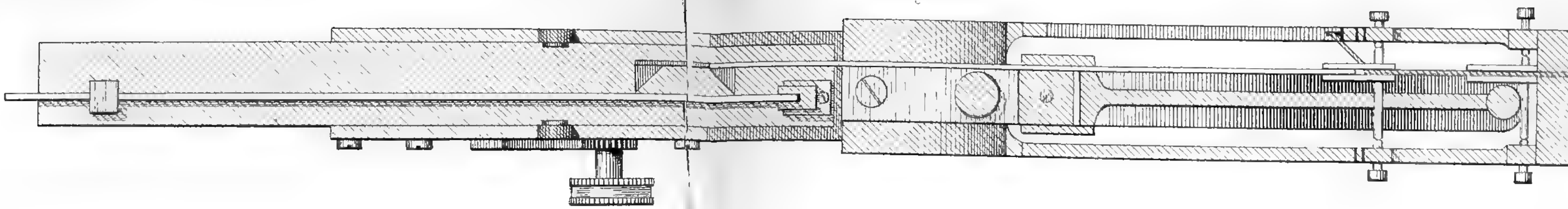
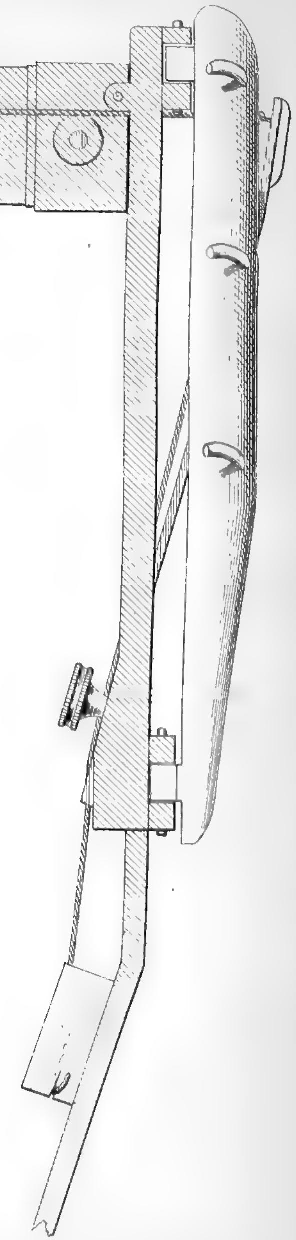



Fig. 2.

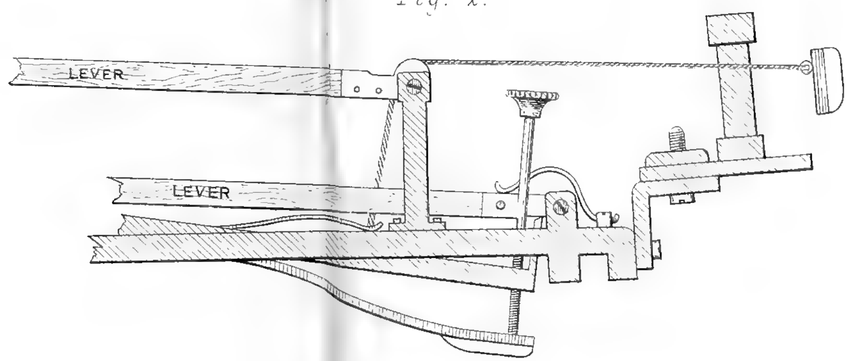
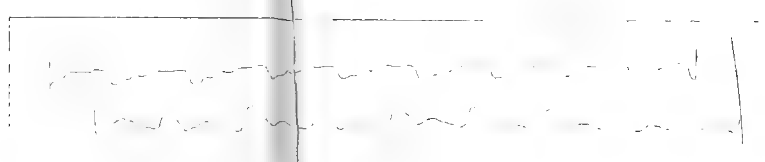
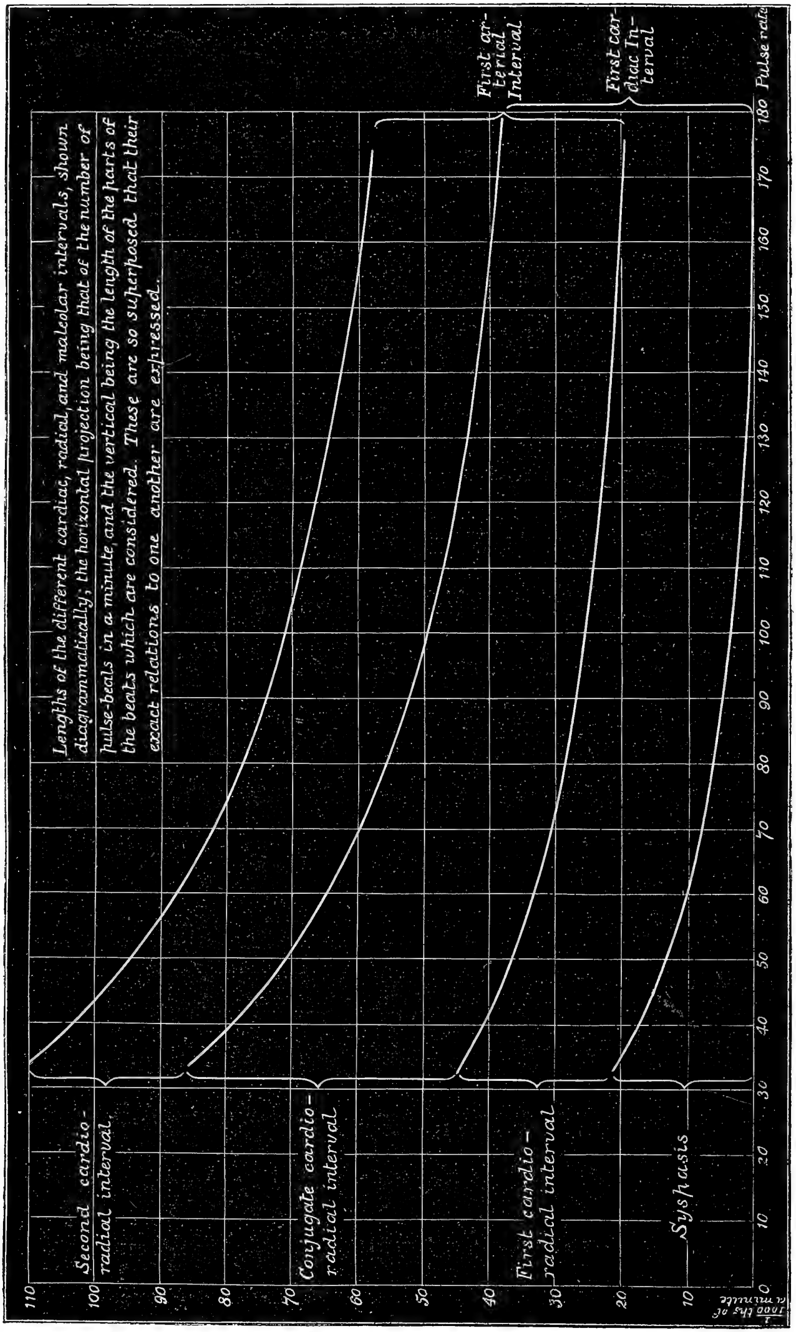


Fig. 3.

Fig. 4.




Lengths of the different cardiac, radial, and maleolar intervals, shown diagrammatically; the horizontal projection being that of the number of pulse-beats in a minute, and the vertical being the length of the parts of the beats which are considered. These are so superposed that their exact relations to one another are expressed.
First arterial Interval
First cardiac Interval
Pulse rate
Second cardio-radial interval
Conjugate cardio-radial interval
First cardio-radial interval
Syphasis
1/1000ths of a minute
0 10 20 30 40 50 60 70 80 90 100 110 120 130 140 150 160 170 180
110 100 90 80 70 60 50 40 30 20 10

January 7, 1875.

JOSEPH DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Remarks on a New Map of the Solar Spectrum." By J. NORMAN LOCKYER, F.R.S. Received November 13, 1874.

I beg permission to lay before the Royal Society a portion of the new map of the solar spectrum referred to in one of my former communications.

It consists of the portion between w. l. 39 and 41.

I have found it necessary, in order to include all the lines visible in my photographs in such a manner that coincidences may be clearly shown, to construct it on four times the scale of Ångström's "Spectre Normal."

The spectra of the following elements have been photographed side by side with the solar spectrum and the coincidences shown:—

Fe, Co, Ni, Mn, Ce, U, Cr, Ba, Sr, Ca, K, Al.

The wave-lengths of new lines in the portion of this spectrum at present completed have been obtained from curves of graphical interpolation. Instead of the reading of a micrometer-scale, a photographic print of the spectrum has been employed in the construction of these curves, the wave-lengths of the principal lines being taken from an unpublished map of the ultra-violet region of the solar spectrum, a copy of which has been kindly placed at my disposal by M. Cornu. The photograph of the solar spectrum from the ultra-violet to beyond F, kindly given to me by Mr. Rutherfurd, has also proved of great service in the present work. I have, in fact, up to the present time, only been able to excel this photograph in the region about H.

From the extreme difficulty of carrying on eye-observations upon the portion of the spectrum now completed, Ångström's map is, of course, very incomplete about this region. The few lines mapped differ slightly in some cases from the positions assigned by Cornu; but the wave-lengths given by the latter observer generally fall into the curve without breaking its symmetry, and these positions have therefore been adopted. The advantage possessed by the photographic method over eye-observation may be estimated from the following numerical comparisons:—

Region of spectrum, 3900–4100.

Number of lines in	Ångström's "Spectre Normal"	39
" "	Ångström's and Thalén's map of the violet part of the solar spectrum	185
" "	Cornu's map	205
" "	new map	518

It will serve further to illustrate the advantages of the photographic method, to compare the number of lines in the spectra of metals already observed with the number of lines of the same metal given by Ångström in the "Spectre Normal."

Region of spectrum, 3900–4100.

Metal.	Lines in new map.	Lines in Thalén's map.
Fe	71	19
Mn	53	12
Co	47	—
Ni	17	—
Ce	163	—
U	18	—
Cr	24	—
Ba	7	—
Sr	5	—
Ca	7	6
K	2	—
Al	2	2
Total	416	39

The purification of the various metallic spectra has at present been only partially effected; but I have seen enough already to convince me of the extreme rigour with which the principle I have already announced may be applied, while, at the same time, there are evidences that the application of it may lead to some results not anticipated in the first instance.

My object in laying these maps before the Society, and presenting this *ad interim* report of progress, is to appeal to some other man of science, if not in England, then in some other country, to come forward to aid in the work, which it is improbable that I, with my small observational means and limited time, can carry to a termination. I reckon that, having regard to routine solar work, it will require another year before the portion from H to G is completely finished, even for the metals the spectra of which are shown in the maps now exhibited. When this is done there will still remain outstanding all the ultra-violet portion, the portion from G to F (both capable of being photographed by short exposure), and the whole of the less-refrangible part (which both Draper and

Rutherfurd have shown can be reached by long exposure with the present processes).

I cannot but think, moreover, that when the light which the spectroscope has already thrown upon molecular action shall be better known, and used as a basis for further inquiry, methods of photography greatly exceeding the present one in rapidity, in the less-refrangible portion of the spectrum, will be developed and utilized in the research.

The map is being drawn by my assistant, Mr. Raphael Meldola (to whom my thanks are due for the skill and patience he has brought to bear upon the work), in the first instance, with more especial reference to the positions, thicknesses, and individualities of the lines; the final revision will consist of an absolute intensity reproduction of the photographs.

II. "On the Spectrum of Coggia's Comet." By William Huggins, D.C.L., LL.D., F.R.S., For. Sec. R.A.S. Received November 13, 1874.

[Plate 6.]

In the years 1866, 1868, and 1871* I had the honour to communicate to the Royal Society some observations with the spectroscope of five small comets, including Encke's comet at its return in 1871.

These observations showed that a great part of the light of these comets was not reflected solar light, but light emitted by the matter of the comets. Further, the coincidence which was found to exist, in the case of three of the comets, between the three bright bands into which their light was resolved by the prism and the spectrum of some compounds of carbon appeared to indicate the presence of that element, in some form, in the cometary matter. The comet now visible, which was detected by M. Coggia, April 17, 1874, is the first bright comet to which the spectroscope has been applied. The following spectroscopic observations of this comet were made from July 1 to July 15.

When the slit of the spectroscope was placed across the nucleus and coma, there was seen in the instrument a broad spectrum, consisting of the three bright bands which were exhibited by Comet II., 1868†, crossed by a linear continuous spectrum from the light of the nucleus.

In the continuous spectrum of the nucleus I was not able to distinguish with certainty any dark lines of absorption, or any bright lines other than the three bright bands.

Besides these spectra, there was also present a faint broad continuous spectrum between and beyond the bright bands.

When the slit was moved on to different parts of the coma, the bright

* Proc. Roy. Soc. vol. xvi. p. 386, vol. xix. p. 490, vol. xx. p. 45, and Phil. Trans. 1868, p. 555.

† Phil. Trans. 1868, plate xxxiii.

bands and the faint continuous spectrum were observed to vary in relative intensity.

When the slit was brought back past the nucleus on to the commencement of the tail, the gaseous spectrum became rapidly fainter, until, at a short distance from the nucleus, the continuous spectrum predominated so strongly that the middle band only, which is the brightest, could be detected on it.

We have presented to us, therefore, by the light of the comet three spectra:—

1. The spectrum of bright bands.
2. The continuous spectrum of the nucleus.
3. The continuous spectrum which accompanies the gaseous spectrum in the coma, and which represents almost entirely the light of the tail.

1. *The Spectrum of Bright Bands.*

The three bright bands were obviously similar in position and character to those which were observed in Comet II., 1868. In that comet the bands could not be resolved into lines; but in the spectrum of the comet now under observation, on some occasions, especially during the early part of July, the three bands were partially resolved into lines. The resolution of the bands was seen most distinctly at the boundary of the coma, where the continuous spectrum was very faint.

The bands appeared to me to be brighter relatively to the continuous spectrum during the early part of the time that the comet was under observation.

On July 7, the bands were compared directly with the spectrum of the induction-spark taken in a current of olefiant gas. I suspected a small shift of all three bands towards the more-refrangible end of the spectrum.

July 8.—I made some measures of the want of coincidence of the less-refrangible edge of the middle and brightest band with the corresponding part of the band in the spectrum of the blue part of a small oil-flame. Afterwards I found that the collimating lens had shifted during the taking of the measures. I repeated the observations on July 13. On this day I estimated the shift of the brightest band to be about $\frac{1}{4}$ of the distance of b^2 to b^3. The other bands appeared to be similarly displaced in relation to the bands of the terrestrial spectrum. The estimation of the amount of displacement was rendered more difficult by the circumstance that the cometary band was not so bright at the less-refrangible limit as was the band in the spectrum of the oil-flame. With this exception, the relative brightness of the different parts of the bands agreed with the corresponding parts of the bands in the terrestrial spectrum.

On the supposition of the identity of the comet's bands with those of the spectrum of carbon, the shift which was observed would indicate a

relative motion of approach of the comet and the earth of about 40 miles per second, a velocity nearly double that of their actual relative motion.

According to a table of the comet's motion, kindly furnished to me by Mr. Hind, F.R.S., the comet was approaching the earth on that day with a velocity of about 24 miles per second. The part of the earth's orbital motion in the direction of the comet may be disregarded, as it was less than a mile per second.

In the foregoing observations the slit was placed on the brightest part of the envelope, close in front of the nucleus. Was any part of the shift due to the motion of the matter within the comet? If the measures taken on July 8, when the lens was found to have shifted, could be regarded as trustworthy, they would indicate a slightly larger shift on that day.

In connexion with the question whether the bright bands were furnished by a vapour containing carbon in some form, it is of importance to notice that the bright line near G which accompanies the three bands in the spectrum of carbon and its compounds appears to be absent in the spectrum of the comet. I took some pains to satisfy myself that this line was not to be detected in the comet's spectrum. If it had been present with the same relative brilliancy which it possesses in the terrestrial spectrum, I should have been able to see it easily. The relative faintness or entire absence of this more-refrangible band might find its explanation possibly in the low temperature of the cometary matter.

If the bands are to be considered as due to carbon, we have to inquire in what form the carbon exists in the matter of the comet. In my paper on Comet II., 1868, I pointed out that though some comets have been exposed to an intense degree of solar radiation, and though carbon might exist possibly in some condition more easily volatilized than those with which we are acquainted on the earth, "still, under the most favourable of known conditions, the solar heat to which the majority of comets are subjected would seem to be inadequate to the production of luminous vapour of carbon." I then stated that the necessity for a very high temperature would be diminished if we were to conceive of the existence in the comet of a compound of carbon and hydrogen which could furnish those bands without undergoing decomposition.

The remarkable connexion of the orbits of comets with those of swarms of meteors, together with the presence of hydrocarbons in many meteorites, would seem to suggest the probability that, if carbon be present in comets, it exists in combination with hydrogen.

I stated, in the paper quoted above, that the spectrum of bright bands was also obtained from cyanogen. In the case of the hydrocarbons, the spectrum of bands was accompanied by the lines of hydrogen; and when cyanogen was employed, the known complex spectrum of nitrogen was present. A spectrum essentially the same, but less complete, was

obtained, together with the known lines of oxygen, when carbonic acid and carbonic oxide were employed.

2. *The Continuous Spectrum of the Nucleus.*

I was not able to satisfy myself of the existence in the continuous spectrum of the nucleus of any dark lines, nor of any bright lines, other than the three bright bands which have been described.

I found that the presence of the bright bands increased the relative brightness of the middle part of the linear continuous spectrum, so as to give an apparently smaller relative amplitude to the red and violet parts of the spectrum. This was particularly noticed to be the case during the first week of July. When some breadth was given to the spectrum by means of a cylindrical lens, the bright bands were clearly distinguished in it, and then the relative brightness of different parts of the continuous spectrum was more nearly that of an ordinary incandescent body. The blue end of the continuous spectrum appeared to fail abruptly a little beyond G, and I was not able to trace the spectrum beyond this point. I took this circumstance at first to show the absence of the violet rays, and consequently a low temperature in the nucleus. Afterwards, when the solar spectrum was reduced to about the brightness of that of the comet, I observed a similar apparent abrupt termination of light at the same part of the spectrum, which is therefore a phenomenon due to the eye of the observer. Although it is probable that the violet rays are absent, or at least not present with any great intensity in the light of the nucleus, this observation of the apparent failure of the spectrum a little beyond G cannot, by itself, be accepted as a trustworthy proof that such is really the case.

When the nucleus was examined in the telescope, it appeared as a well-defined minute point of light of great brilliancy. I suspected at times a sort of intermittent flashing in the bright point. The nucleus suggested to me an object on fire, of which the substance was not uniform in composition, so that at intervals it burned with a more vivid light. On July 6th the diameter of the nucleus, when measured with a power of 800, was $1''\cdot8$. On July 13th the measure was nearly double, viz. $3''$; but at this time the point of light was less defined. On July 15th the nucleus appeared elongated towards the following side of the comet, at an angle of about 40° to the comet's axis.

The nucleus appeared of an orange-colour. This may be due in part to the effect of contrast with the greenish light of the coma. Sir John Herschel described the head of the comet of 1811 to be of a greenish or bluish-green colour, while the central point appeared to be of a pale ruddy tint. The elder Struve's representations of Halley's comet, at its appearance in 1835, are coloured green, and the nucleus is coloured reddish yellow. He describes the nucleus on October 9 thus, "Der Kern zeigte sich wie eine kleine, etwas ins gelbliche spielende, glühende Kohle von länglicher Form." Dr. Winnecke describes similar colours in the bright

comet of 1862:—"Die Farbe des Strahls erscheint mir gelbröthlich; die des umgebenden Nebels (vielleicht aus Contrast) mattbläulich. . . . Die Farbe der Ausströmung erscheint mir gelblich; die Coma hat bläuliches Licht."

3. *The Continuous Spectrum which accompanies the Gaseous Spectrum.*

This spectrum was observed in every part of the coma; near its boundary, and in the dark space behind the nucleus, the continuous spectrum became so faint as to be detected with difficulty, at the same time that the bright bands were distinctly visible.

The greater brightness of some parts of the envelopes and of the coma appeared to be due for the most part to the presence of a larger quantity of the matter which gives a continuous spectrum. When the slit was placed on these brighter parts of the comet, the gaseous spectrum did not become brighter in the same degree, but there was an increase in the brightness of the continuous spectrum.

Behind the nucleus, the bright bands became fainter relatively to the accompanying continuous spectrum, until the brightest band only could be detected. The more distant parts of the tail give probably a continuous spectrum only.

In the coma there was seen occasionally a remarkable inequality in the brightness of the continuous spectrum between the bands. On some occasions the light between the first and second band was bright, while in the other parts of the spectrum the light was faint.

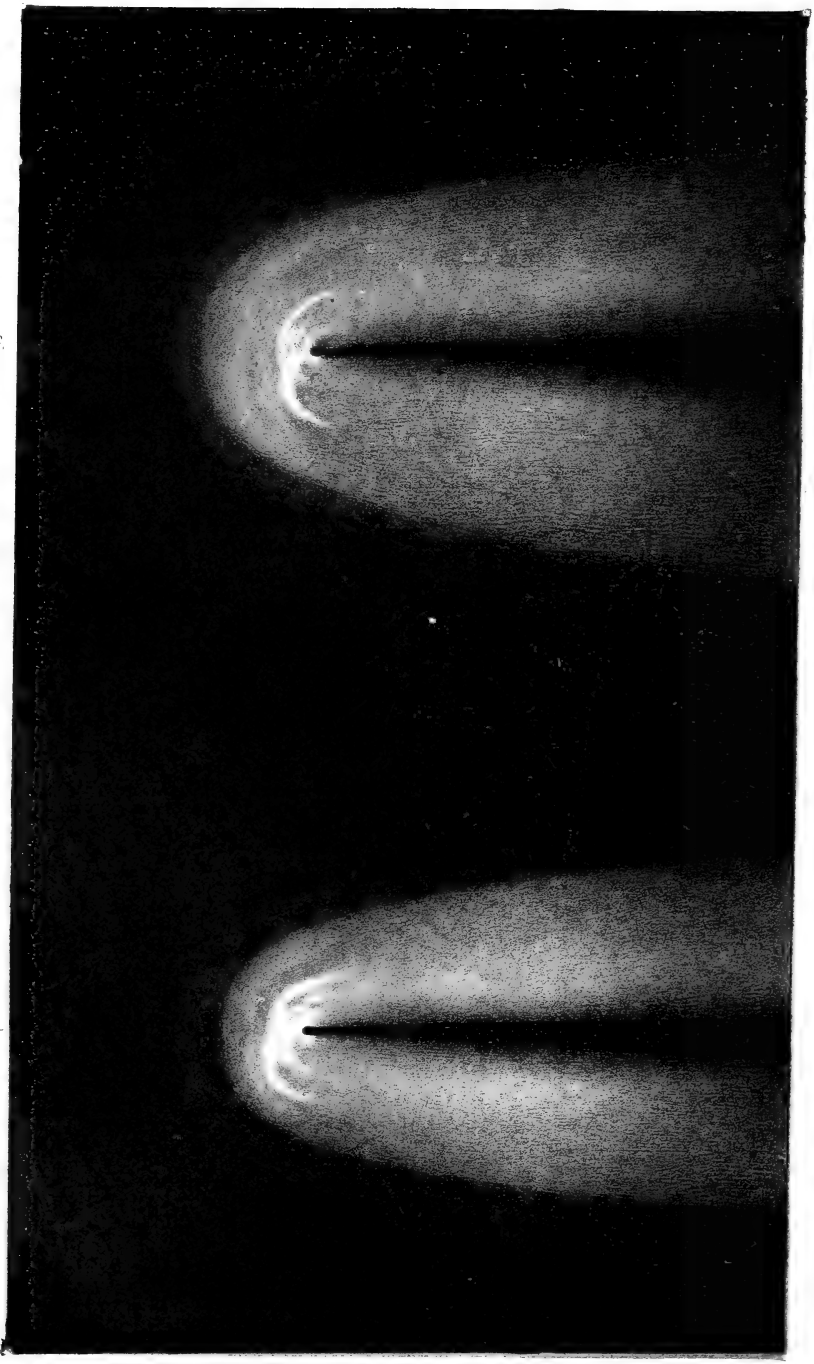
On several evenings I satisfied myself that polarized light was present in every part of the comet. I do not think that the proportion of polarized light exceeded $\frac{1}{5}$ of the total light. The polarization, as exhibited by the partial extinction of one of the images formed by a double-image prism, appeared to be more marked in the tail. It must be remembered that such would appear to be the case to some extent even if the proportion were not really greater, because the same proportional diminution in a faint object is more appreciated by the eye. Still there was probably a relatively large proportion of polarized light in the tail.

The reflected solar light would account for a large part of the continuous spectrum. To what source are we to ascribe the remaining light which the prism resolves into a continuous spectrum? Is it due to reflection from discrete particles, too large relatively to the wave-lengths of the light for polarization to take place? or is it due to incandescent solid particles? From the coexistence of the band-spectrum, we can scarcely think of distinct masses of gas dense enough to give a continuous spectrum.

The difficulty which presents itself in accounting for sufficient heat to maintain this matter and the nucleus in a state of incandescence has also to be encountered in respect of the gaseous matter which emits the light which is resolved into the bright bands.

July 13

July 14



W. Huggins del.
W. H. Wesley lith.

W. West & Co. imp.

The solar radiation to which the comet was subjected would be inadequate to account for this state of things directly. Is there chemical action set up within the comet by the sun's heat? Is the comet's light due to electricity in any form excited by the effect of the solar radiation upon the matter of the comet? Are we to look for the source of the light to the friction of the particles of the cometary matter which has been thrown into violent agitation by the comet's approach to the sun?

The comet was unfavourably situated for examination from my observatory, as it was seen on a background of sky illuminated by the lights of London; and as it approached the horizon, it became partially concealed by the chimneys of some neighbouring houses. Nearly the whole of the time that the comet was visible was consumed in the observations with the spectroscope; and a few sketches only were made of the appearances presented by the head of the comet in the telescope.

The two sketches which accompany this note (Plate 6) were made on July 13 and 14. On the latter evening the fainter parts of the coma, which are wanting in the sketch, were rendered invisible by the bright background of sky. Two of the phases presented by the intersection of the envelopes are shown in the sketches. The narrow black channel behind the nucleus passed on the right-hand side of the nucleus (as seen in the telescope), where it terminated in a small round extension, presenting something of the appearance of a black pin, with its head by the side of the nucleus.

III. "On Acoustic Reversibility." By J. Tyndall, D.C.L., LL.D., F.R.S. Received December 31, 1874.

On the 21st and 22nd of June, 1822, a Commission appointed by the Bureau of Longitudes of France executed a celebrated series of experiments on the velocity of sound. Two stations had been chosen, the one at Villejuif, the other at Montlhéry, both lying south of Paris, and 11·6 miles distant from each other. Prony, Mathieu, and Arago were the observers at Villejuif, while Humboldt, Bouvard, and Gay-Lussac were at Montlhéry. Guns, charged sometimes with 2 lbs. and sometimes with 3 lbs. of powder, were fired at both stations, and the velocity was deduced from the interval between the appearance of the flash and the arrival of the sound.

On this memorable occasion an observation was made which, as far as I know, has remained a scientific enigma to the present hour. It was noticed that while every report of the cannon fired at Montlhéry was heard with the greatest distinctness at Villejuif, by far the greater number of the reports from Villejuif failed to reach Montlhéry. Had wind existed, and had it blown from Montlhéry to Villejuif, it would

have been recognized as the cause of the observed difference; but the air at the time was calm, the slight motion of translation actually existing being from Villejuif towards Montlhéry, or against the direction in which the sound was best heard.

So marked was the difference in transmissive power between the two directions, that on the 22nd of June, while every shot fired at Montlhéry was heard "à merveille" at Villejuif, but one shot out of twelve fired at Villejuif was heard, and that feebly, at the other station.

With the caution which characterized him on other occasions, and which has been referred to admiringly by Faraday*, Arago made no attempt to explain this anomaly. His words are:—"Quant aux différences si remarquables d'intensité que le bruit du canon a toujours présentés suivant qu'il se propageaient du nord au sud entre Villejuif et Montlhéry, ou du sud au nord entre cette seconde station et la première; nous ne chercherons pas aujourd'hui à l'expliquer, parce que nous ne pourrions offrir au lecteur que des conjectures denuées de preuves"†.

I have tried, after much perplexity of thought, to bring this subject within the range of experiment, and have now to submit to the Royal Society a possible solution of the enigma. The first step was to ascertain whether the sensitive flame referred to in my recent paper in the Philosophical Transactions could be safely employed in experiments on the mutual reversibility of a source of sound and an object on which the sound impinges. Now the sensitive flame usually employed by me measures from 18 to 24 inches in height, while the reed employed as a source of sound is less than a square quarter of an inch in area. If, therefore, the whole flame, or the pipe which fed it, were sensitive to sonorous vibrations, strict experiments on reversibility with the reed and flame might be difficult, if not impossible. Hence my desire to learn whether the seat of sensitiveness was so localized in the flame as to render the contemplated interchange of flame and reed permissible.

The flame being placed behind a cardboard screen, the shank of a funnel passed through a hole in the cardboard was directed upon the middle of the flame. The sound-waves issuing from the vibrating reed placed within the funnel produced no sensible effect upon the flame. Shifting the funnel so as to direct its shank upon the root of the flame, the action was violent.

To augment the precision of the experiment, the funnel was connected with a glass tube 3 feet long and half an inch in diameter, the object being to weaken by distance the effect of the waves diffracted round the edge of the funnel, and to permit those only which passed through the glass tube to act upon the flame.

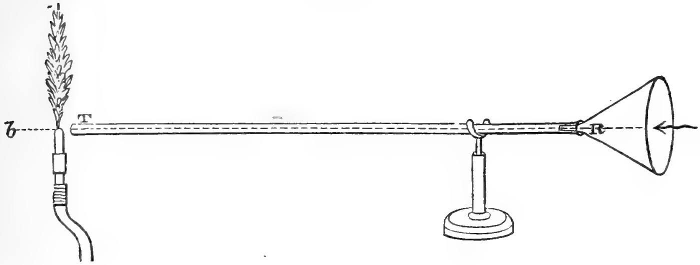
Presenting the end of the tube to the orifice of the burner (*b*, fig. 1),

* Researches in Chemistry and Physics, p. 484.

† Connaissance des Temps, 1825, p. 370.

or the orifice to the end of the tube, the flame was violently agitated by the sounding-reed, R. On shifting the tube, or the burner, so as to concentrate the sound on a portion of the flame about half an

Fig. 1.



inch above the orifice, the action was *nil*. Concentrating the sound upon the burner itself about half an inch below its orifice, there was no action.

These experiments demonstrate the localization of "the seat of sensitiveness," and they prove the flame to be an appropriate instrument for the contemplated experiments on reversibility.

The experiments proceeded thus:—The sensitive flame being placed close behind a screen of cardboard 18 inches high by 12 inches wide, a vibrating reed, standing at the same height as the root of the flame, was placed at a distance of 6 feet on the other side of the screen. The sound of the reed, in this position, produced a strong agitation of the flame.

The whole upper half of the flame was here visible from the reed; hence the necessity of the foregoing experiments to prove the action of the sound on the upper portion of the flame to be *nil*, and that the waves had really to bend round the edge of the screen so as to reach the seat of sensitiveness in the neighbourhood of the burner.

The positions of the flame and reed were reversed, the latter being now close behind the screen, and the former at a distance of 6 feet from it. The sonorous vibrations were without sensible action upon the flame.

The experiment was repeated and varied in many ways. Screens of various sizes were employed; and instead of reversing the positions of the flame and reed, the screen was moved so as to bring, in some experiments the flame, and in other experiments the reed, close behind it. Care was also taken that no reflected sound from the walls or ceiling of the laboratory, or from the body of the experimenter, should have any thing to do with the effect. In all cases it was shown that the sound was effective when the reed was at a distance from the screen and the flame close

behind it; while the action was insensible when these positions were reversed.

Thus, let *s e*, fig. 2, be a vertical section of the screen. When the reed was at A and the flame at B there was no action; when the reed was at B

Fig. 2.

and the flame at A the action was decided. It may be added that the vibrations communicated to the screen itself, and from it to the air beyond it, were without effect; for when the reed, which at B is effectual, was shifted to C, where its action on the screen was greatly augmented, it ceased to have any action on the flame at A.

We are now, I think, prepared to consider the failure of reversibility in the larger experiments of 1822. Happily an incidental observation of great significance comes here to our aid. It was observed and recorded at the time that while the reports of the guns at Villejuif were without echoes, a roll of echoes, lasting from 20 to 25 seconds, accompanied every shot at Montlhéry, being heard by the observers there. Arago, the writer of the Report, referred these echoes to reflection from the clouds, an explanation which I think we are entitled to regard as problematical. The report says that "tous les coups tirés à Montlhéry y étaient *accompagnés* d'un roulement semblable à celui du tonnerre." I have italicized a very significant word—a word which fairly applies to our experiments on gun-sounds at the South Foreland, where there was no sensible solution of continuity between explosion and echo, but which could hardly apply to echoes coming from the clouds. For supposing the clouds to be only a mile distant, the sound and its echo would have been separated by an interval of nearly ten seconds. But there is no mention of any interval; and had such existed, surely the word "followed," instead of "accompanied," would have been the one employed. The echoes, moreover, appear to have been *continuous*, while the clouds observed seem to have been *separate*. "Ces phénomènes," says Arago, "n'ont jamais eu lieu qu'au moment de l'apparition de quelques nuages." But from separate clouds a continuous roll of echoes could hardly come. When to this is added the experimental fact that clouds far denser than any ever formed in the atmosphere are demonstrably incapable of sensibly reflecting sound, while cloudless air, which Arago pronounced echoless, has been proved capable of powerfully reflecting it, I think we have

strong reason to question the hypothesis of the illustrious French philosopher.

And considering the hundreds of shots fired at the South Foreland, with the attention specially directed to the aerial echoes, when no single case occurred in which echoes of measurable duration did not accompany the report of the gun, I think Arago's statement that at Villejuif no echoes were heard when the sky was clear must simply mean that they vanished with great rapidity. Unless the attention were specially directed to the point, a slight prolongation of the cannon-sound might well escape observation; and it would be all the more likely to do so if the echoes were so loud and prompt as to form apparently part and parcel of the direct sound.

I should be very loth to transgress here the limits of fair criticism, or to throw doubt, without good reason, on the recorded observations of an eminent man. Still, taking into account what has been just stated, and remembering that the minds of Arago and his colleagues were occupied by a totally different problem (that the echoes were an incident rather than an object of observation), I think we may justly consider the sound which he called "instantaneous" as one whose aerial echoes did not differentiate themselves from the direct sound by any noticeable fall of intensity, and which rapidly died into silence.

Turning now to the observations at Montlhéry, we are struck by the extraordinary duration of the echoes heard at that station. At the South Foreland the charge habitually fired was equal to the largest of those employed by the French philosophers; but on no occasion did the gun-sounds produce echoes approaching to 20 or 25 seconds' duration. It rarely reached half this amount. Even the syren-echoes, which were more remarkable and more long-continued than those of the gun, never reached the duration of the Montlhéry echoes. The nearest approach to it was on the 17th of October, 1873, when the syren-echoes required 15 seconds to subside into silence.

On this same day, moreover (and this is a point of marked significance), the transmitted sound reached its maximum range, the gun-sounds being heard at the Quenocs buoy, which is 16½ nautical miles from the South Foreland. I have already stated that the duration of the air-echoes indicates "the atmospheric depths" from which they come*. An optical analogy may help us here. Let light fall upon chalk, the light is wholly scattered by the superficial particles; let the chalk be powdered and mixed with water, light reaches the observer from a far greater depth of the turbid liquid. The solid chalk typifies the action of exceedingly dense acoustic clouds; the chalk and water that of clouds of moderate density. In the one case we have echoes of short, in the other echoes of long duration. These considerations prepare us for the inference that Montlhéry, on the occasion referred to, must have been sur-

* Phil. Trans. 1874, pt. i. p. 202.

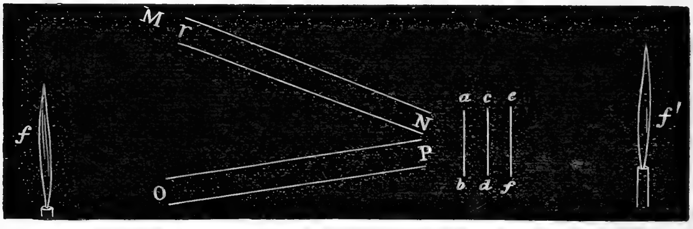
rounded by a highly diacoustic atmosphere; while the shortness of the echoes at Villejuif shows the atmosphere surrounding that station to have been acoustically opaque.

Have we any clue to the cause of the opacity? I think we have. Villejuif is close to Paris, and over it, with the observed light wind, was slowly wafted the air from the city. Thousands of chimneys to windward of Villejuif were discharging their heated currents; so that an atmosphere non-homogeneous in a high degree must have surrounded that station. At no great height in the atmosphere the equilibrium of temperature would be established. The non-homogeneous air surrounding Villejuif is experimentally typified by our screen with the source of sound close behind it, the upper edge of the screen representing the place where equilibrium of temperature was established in the atmosphere above the station. In virtue of its proximity to the screen, the echoes from our sounding-reed would, in the case here supposed, so blend with the direct sound as to be practically indistinguishable from it, as the echoes at Villejuif followed the direct sound so hotly, and vanished so rapidly, that they escaped observation. And as our sensitive flame, at a distance, failed to be affected by the sounding body placed close behind the card-board screen, so, I take it, did the observers at Montlhéry fail to hear the sounds of the Villejuif gun. This is the explanation of Arago's difficulty which I have the honour to submit to the Royal Society.

Received January 4, 1875.

Something further may be done towards the experimental elucidation of this subject. The facility with which sounds pass through textile fabrics has been already illustrated*, a layer of cambric or calico, or even of thick flannel or baize, being found competent to intercept but a fraction of the sound from a vibrating reed. Such a layer of calico may be taken to represent a layer of air differentiated from its neighbours by temperature or moisture; while a succession of such sheets of calico may be taken to represent successive layers of non-homogeneous air.

Fig. 3.



Two tin tubes (M N and O P, fig. 3) with open ends are placed so

* Phil. Trans. 1874, pt. i. p. 208.

as to form an acute angle with each other. At the end of one is the vibrating reed r; opposite the end of the other, and in the prolongation of P O, is the sensitive flame f, a second sensitive flame (f') being placed in the continuation of the axis of M N. On sounding the reed, the direct sound through M N agitates the flame f'. Introducing the square of calico ab at the proper angle, a slight decrease of the action on f' is noticed, and the feeble echo from ab produces a barely perceptible agitation of the flame f. Adding another square, cd, the sound transmitted by ab impinges on cd; it is partially echoed, returns through ab, passes along P O, and still further agitates the flame f. Adding a third square, ef, the reflected sound is still further augmented, every accession to the echo being accompanied by a corresponding withdrawal of the vibrations from f' and a consequent stilling of that flame.

With thinner calico or cambric it would require a greater number of layers to intercept the entire sound; hence with such cambric we should have echoes returned from a greater distance, and therefore of greater duration. Eight layers of the calico employed in these experiments, stretched on a wire frame and placed close together as a kind of pad, may be taken to represent a very dense acoustic cloud. Such a pad, placed at the proper angle beyond N, cuts off the sound, which in its absence reaches f', almost as effectually as an impervious solid plate*: the flame f' is thereby stilled, while f is far more powerfully agitated than by the reflection from a single layer. With the source of sound close at hand, the echoes from such a pad would be of insensible duration. Thus close at hand do I suppose the acoustic clouds surrounding Villejuif to have been, a similar shortness of echo being the consequence.

A further step is here taken in the illustration of the analogy between light and sound. Our pad acts chiefly by internal reflection. The sound from the reed is a composite one, made up of partial sounds differing in pitch. If these sounds be ejected from the pad in their pristine proportions, the pad is acoustically *white*; if they return with their proportions altered, the pad is acoustically *coloured*.

In these experiments my assistant, Mr. Cottrell, has rendered me material assistance.

* January 13th.—Since this was written I have sent the sound through fifteen layers of calico, and echoed it back through the same layers, in strength sufficient to agitate the flame. Thirty layers were here crossed by the sound.

January 14, 1875.

JOSEPH DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On a Class of Identical Relations in the Theory of Elliptic Functions." By J. W. L. GLAISHER, M.A., Fellow of Trinity College, Cambridge. Communicated by JAMES GLAISHER, F.R.S. Received November 23, 1874.

(Abstract.)

The object of the memoir is to notice certain forms into which the primary elliptic functions admit of being thrown, and to discuss the identical relations to which they give rise. These latter, it is shown, can be obtained directly by the aid of Fourier's theorem, or in a less straightforward manner by ordinary algebra.

Thus, *ex. gr.*, consider the cosine-amplitude: it is shown that we have the formula

$$\operatorname{cosam} 2\mathrm{K}x = \frac{\pi}{k\mathrm{K}'}\left\{\frac{1}{r^x+r^{-x}} - \frac{1}{r^{x-1}+r^{-(x-1)}} - \frac{1}{r^{x+1}+r^{-(x+1)}} \right.$$
$$\left. + \frac{1}{r^{x-2}+r^{-(x-2)}} + \frac{1}{r^{x+2}+r^{-(x+2)}} - \&c. \right\}$$

(r being $= e^{-\frac{\pi\mathrm{K}}{\mathrm{K}'}}$), which is noticeable as being of the form

$$\phi x - \phi(x-1) - \phi(x+1) + \&c.,$$

and therefore an analogue of

$$\pi \operatorname{cosec} \pi x = \frac{1}{x} - \frac{1}{x-1} - \frac{1}{x+1} + \frac{1}{x-2} + \frac{1}{x+2} - \&c.$$

This form of the cosine-amplitude gives rise to the identical equation

$$\operatorname{sech} x - \operatorname{sech}(x-\mu) - \operatorname{sech}(x+\mu) + \operatorname{sech}(x-2\mu) + \operatorname{sech}(x+2\mu) - \&c.$$
$$= \frac{2\pi}{\mu}\left\{\operatorname{sech}\frac{\pi^2}{2\mu}\cos\frac{\pi x}{\mu} + \operatorname{sech}\frac{3\pi^2}{2\mu}\cos\frac{3\pi x}{\mu} + \&c.\right\} \quad . \quad . \quad . \quad . \quad (1)$$

(sech being the hyperbolic secant). The result (1) is deducible at once from the integral

$$\int^{\infty} \operatorname{sech} x \cos nx \, dx = \frac{\pi}{2} \operatorname{sech} \frac{n\pi}{2};$$

and it is remarked that all evaluable integrals of the forms

$$\int_0^{\infty} \phi x \cos nx \, dx, \quad \int_0^{\infty} \psi x \sin nx \, dx$$

(ϕ even and ψ uneven) give rise to identities of the same class as (1), and which, it appears, are all elliptic-function relations. It is alsop ointed out how (1) and the other similar identities discussed in the memoir admit of being simply established by ordinary algebra and trigonometry in two different ways.

Other identities and formulæ are noticed and compared; *ex. gr.*, for the cosine-amplitude, writing

$$x=\frac{\pi u}{2\mathrm{K}},\quad z=\frac{\pi u}{2\mathrm{K}'},\quad q=e^{-\frac{\pi\mathrm{K}'}{\mathrm{K}}}=e^{-\mu},\quad r=e^{-\frac{\pi\mathrm{K}}{\mathrm{K}'}}=e^{-\nu},$$

we have

$$\begin{aligned}
\operatorname{cosam} u &= \frac{2\pi}{k\mathrm{K}}\left\{\frac{q^{\frac{1}{2}}}{1+q}\cos x+\frac{q^{\frac{3}{2}}}{1+q^3}\cos 3x+\frac{q^{\frac{5}{2}}}{1+q^5}\cos 5x+\text{\&c.}\right\}\\
&=\frac{2\pi}{k\mathrm{K}}\cos x\left\{\frac{q^{\frac{1}{2}}(1-q)}{1-2q\cos 2x+q^2}-\frac{q^{\frac{3}{2}}(1-q^3)}{1-2q^3\cos 2x+q^6}+\text{\&c.}\right\}\\
&=\frac{\pi}{2k\mathrm{K}'}\{\operatorname{sech} z-\operatorname{sech}(z-\nu)-\operatorname{sech}(z+\nu)+\text{\&c.}\}\\
&=\frac{\pi}{2k\mathrm{K}'}\left\{\operatorname{sech} z-4\cosh x\left(\frac{\cosh\nu}{\cosh 2z+\cos 2\nu}\right.\right.\\
&\qquad\left.\left.-\frac{\cosh 2\nu}{\cosh 4z+\cosh 4\nu}+\text{\&c.}\right)\right\}\\
&=\frac{\pi}{k\mathrm{K}'}\left\{\frac{\sinh(\frac{1}{2}\nu-z)}{\cosh\frac{1}{2}\nu}-\frac{\sinh 3(\frac{1}{2}\nu-z)}{\cosh\frac{3}{2}\nu}+\frac{\sinh 5(\frac{1}{2}\nu-z)}{\cosh\frac{5}{2}\nu}-\text{\&c.}\right\}\\
&=\frac{\pi}{2k\mathrm{K}'}\left\{\operatorname{sech} z-\frac{4r}{1+r}\cosh z+\frac{4r^3}{1+r^3}\cosh 3z-\text{\&c.}\right\};
\end{aligned}$$

so that if x, z, μ, ν be any four quantities subject to the relations

$$\mu\nu=\pi^2,\quad z=\frac{\pi x}{\mu}\left(\text{so that } x=\frac{\pi z}{\nu}\right),$$

he identities are

$$\begin{aligned}
&\operatorname{sech} x-\operatorname{sech}(x-\mu)-\operatorname{sech}(x+\mu)+\operatorname{sech}(x-2\mu)\\
&\qquad+\operatorname{sech}(x+2\mu)-\text{\&c.}\\
&=\operatorname{sech} x-4\cosh x\left(\frac{\cosh\mu}{\cosh 2x+\cosh 2\mu}-\frac{\cosh 2\mu}{\cosh 4x+\cosh 4\mu}+\text{\&c.}\right)\\
&=2\left\{\frac{\sinh(\frac{1}{2}\mu-x)}{\cosh\frac{1}{2}\mu}-\frac{\sinh 3(\frac{1}{2}\mu-x)}{\cosh\frac{3}{2}\mu}\right.\\
&\qquad\left.+\frac{\sinh 5(\frac{1}{2}\mu-x)}{\cosh\frac{5}{2}\mu}-\text{\&c.}\right\}\quad\ldots\ldots\ldots\ldots\quad(2)\\
&=\operatorname{sech} x-\frac{4\cosh x}{e^{\mu}+1}+\frac{4\cosh 3x}{e^{3\mu}+1}-\text{\&c.}\\
&=\frac{2\pi}{\mu}\left\{\frac{\cosh z}{\cosh\frac{1}{2}\nu}+\frac{\cosh 3z}{\cosh\frac{3}{2}\nu}+\frac{\cosh 5z}{\cosh\frac{5}{2}\nu}+\text{\&c.}\right\}\\
&=\frac{2\pi}{\mu}\cos z\left\{\frac{\sinh\frac{1}{2}\nu}{\sin^2 x+\sinh^2\frac{1}{2}\nu}-\frac{\sinh\frac{3}{2}\nu}{\sin^2 x+\sinh^2\frac{3}{2}\nu}+\text{\&c.}\right\}
\end{aligned}$$

(see Schellbach, 'Die Lehre von den elliptischen Integralen,' 1864, p. 113). In (2), x must be less than μ.

The two formulæ obtained in the memoir,

$$\tanh x + \tanh(x-\mu) + \tanh(x+\mu) + \&c.$$
$$= \frac{2x}{\mu} + \frac{2\pi}{\mu}\left\{\frac{\sin 2z}{\sinh \nu} + \frac{\sin 4z}{\sinh 2\nu} + \&c.\right\},$$

$$\coth x + \coth(x-\mu) + \coth(x+\mu) + \&c.$$
$$= \frac{2x}{\mu} + \frac{\pi}{\mu}\left\{\cot z + \frac{4\sin 2z}{e^{2\nu}-1} + \frac{4\sin 4z}{e^{4\nu}-1} + \&c.,\right.$$

are considered in some detail, and an algebraical proof is added of Abel's identity ('Œuvres,' t. i. p. 307),

$$\frac{1}{\sqrt[24]{q}}(1+q)(1+q^3)(1+q^5)\ldots = \frac{1}{\sqrt[24]{r}}(1+r)(1+r^3)(1+r^5)\ldots.$$

II. "On some remarkable Changes produced in Iron and Steel by the Action of Hydrogen and Acids." By William H. Johnson, B.Sc. Communicated by Prof. Sir William Thomson, LL.D., F.R.S. Received December 7, 1874.

Some three years ago my attention was called to a remarkable change in some of the physical properties of iron caused by its temporary immersion in hydrochloric and sulphuric acids. This change is at once made evident to any one by the extraordinary decrease in toughness and breaking-strain of the iron so treated, and is all the more remarkable as it is not permanent, but only temporary in character, for with lapse of time the metal slowly regains its original toughness and strength. With a view of ascertaining the cause and degree of this change, I have from time to time made a number of experiments, some of which were carried out on a large scale in an iron-works where quantities of sulphuric and hydrochloric acids are used to remove the coating of oxide from iron wire, preparatory to drawing it. Many of these experiments have been already described in a somewhat desultory form in the 'Proceedings of the Literary and Philosophical Society of Manchester' for Jan. 7th, March 4th, Dec. 30th, 1873, Jan. 13th, March 10th and 24th, 1874.

As mentioned before, I first noticed that iron wire became more brittle after a few minutes' immersion (half a minute will sometimes suffice) in strong hydrochloric or dilute sulphuric acid—a piece breaking after being bent once on itself, while before immersion it would bear bending on itself and back again two or three times before breaking. But perhaps the most remarkable phenomenon was, that if, while still hot from the effort of breaking, the fractured part was wetted, it appeared to froth,

copious bubbles of gas being given off from the whole surface of the fracture for 30 to 40 seconds, or even longer, making the water on the fractured surface appear to boil violently. This frothing is increased by any thing that augments the heat produced by fracture; in fact it is necessary that the fracture be more or less warm to cause the escape of bubbles; for if the wire be nicked and broken short without generating any heat, few or no bubbles will be seen. By further experiment I found that other acids, such as acetic, had the same effect on iron as those first used; and it became evident that any acid which liberates hydrogen by its action on iron is able to produce them. Nitric acid, which under usual conditions does not liberate hydrogen by its action on iron, is, however, without effect. The frothing, the diminution of toughness, and all other changes caused by immersion in acid are, as a rule, only temporary; for after an exposure to a temperature of about 16° C. for three days, or of 200° C. for half a day, the wire will be found to have regained its original toughness, and no bubbles, or any sign of evolution of gas, will be seen, as before, on moistening the fracture. The bubbles also cease to be visible long before the wire has recovered its original toughness or elasticity. Immersion in water, particularly if warm, hastens the restoration of toughness, and numerous bubbles may be seen to arise from the iron when first immersed. If a little caustic soda, or other alkali, be added to the water in which the iron is laid, its recovery is still further hastened, as it neutralizes a film of acid which seems to adhere to the surface of all iron which has been attacked by acid.

It seems at first remarkable that steel does not froth when fractured after immersion in acid, under the same conditions as will produce a violent evolution of gas with iron; and yet the action of acids on steel is more rapid, more marked, and more permanent than on iron. The decrease in toughness is such that a piece of steel which, previous to immersion in hydrochloric or sulphuric acid, would stand bending on itself and back two or three times, will break short off like a pipe-stem when bent. So great is the influence of acid, in fact, that 10 minutes' immersion in dilute sulphuric acid will sometimes cause a coil of highly carbonized tempered cast-steel wire to break of itself into several pieces while in the liquid.

The amount of carbon in the steel appears, moreover, to be connected with the action of acid; for in mild Bessemer steels, containing about 0·20 to 0·25 per cent. of carbon, the change is a very little more marked than in iron, even frothing being apparent after prolonged immersion. With an increased percentage of carbon the action, however, is more marked and of longer duration. Half an hour's immersion in hydrochloric acid will make a piece of steel containing, say, 0·60 per cent. of carbon break with a much darker-coloured fracture, and render it so brittle that no amount of exposure to the air or heat will ever completely restore it. On hardened and tempered steel the decrease in toughness

produced by immersion in acid is greater and more rapid than with the same steel in a soft state.

Suspecting that the absence of frothing on the surface of steel after immersion in acid might arise from the bubbles of gas given off being so small as to be invisible to the naked eye, I examined the moistened fracture of a piece of steel under a microscope with a power of 250 diameters. My expectations were fulfilled, for numbers of minute bubbles were seen to rise from parts of the moistened fracture. It appears that the fibrous and open structure of iron allows any gas which has been occluded in its substance to pass more easily to the surface of the fracture than will the close, unfibrous, homogeneous structure of steel; consequently the evolution of gas will not be so rapid with steel as with iron. Moreover the fracture of steel presents an almost infinite number of small points, all favourable to the rapid evolution of small bubbles invisible to the naked eye. Iron wire, on the other hand, breaks with a fibrous mossy fracture, which will retain the small bubbles until they have grown sufficiently large to be visible to the naked eye. Hence the frothing in iron and its absence in steel.

The following experiments were made to ascertain if there was any appreciable increase in weight in iron which frothed over the same iron when in its usual state. The pieces of iron and steel wire, after immersion in acid, were well washed in cold water, and when dry weighed, this being the weight when the metal contained the gas. Subsequently they were heated several hours in an oven to restore them as far as possible, and again weighed. The result of the last weighing, in every instance, must always have been a little too large, as it was found impossible to prevent a thin film of rust forming on the surface of the metal when being heated in the oven. Notwithstanding this, the results showed in every case a gain in weight after immersion in acid. After five hours' immersion in acid the average gain in weight for mild steel, charcoal-iron, and common iron was in

Hydrochloric acid	·028 per cent.
Sulphuric acid	·036 ,,

The steel gained considerably the most—a result well worthy of notice; for we shall see that the tensile strain and elasticity of steel are far more affected by the presence of absorbed hydrogen than iron under like conditions—probably in part because more gas is occluded by steel than iron (a conclusion which the greater increase in weight of steel, in comparison with iron, bears out), and in part owing to the different molecular structure of steel.

I hope at some future time to ascertain, if possible, if this gain in weight is entirely due to occluded hydrogen, or whether also to absorption of acid to a greater or less extent.

Having examined in detail some of the effects of immersion in acid

upon iron and steel, we will now more closely consider them with the object of discovering the cause.

It might at first sight be thought that the frothing could be explained on the supposition that by the action of acid, iron is thrown into what may be called the "active state," in opposition to the so-called passive state caused by nitric acid, and that in this "active state" it is able to decompose water at the ordinary temperature, forming oxide of iron and bubbles of free hydrogen. The facts, however, do not bear out this theory, as the bubbles are still seen if oil be employed instead of water; and no matter how numerous the bubbles, the closest examination fails to show any formation of oxide. Again, the frothing is greater from the long end than from the short end of the piece of wire, whereas, if due to oxidation, it should be the same at both ends.

Now the following facts make it certain that hydrogen is either the sole cause of the changes produced in iron by some acids, or is inseparably connected therewith:—

1st. Only those acids which evolve hydrogen by their action on iron produce any change in iron and steel, nitric acid (which does not liberate hydrogen except under particular conditions) having no effect.

2nd. It is difficult to collect the bubbles which form the froth on the moistened fracture of a piece of iron in sufficient quantity for analysis; but by putting a coil of wire, previously steeped in acid, into hot water under a bell-jar, the bubbles of gas evolved by the iron may be collected, and will be found to burn with the characteristic flame of hydrogen.

Hence it is probable that iron and steel, when placed in hydrochloric, sulphuric, or other acid, absorb some of the nascent hydrogen generated by the action of the acid, thus forming what, for lack of a better term, may be called an alloy* of iron and hydrogen. This alloy may be compared to that formed when zinc is amalgamated with mercury; and just as in process of time the mercury disconnects itself from the zinc, appearing in globules on its surface, so hydrogen gradually disengages itself from the iron—a movement which is greatly facilitated by heat, as is natural to expect.

The analogy may be carried still further; for as amalgamated zinc is made brittle in consequence of the pores or interstices between the molecules of the metal being filled up by mercury, motion of one molecule over another being then impeded, so in like manner iron becomes brittle when its pores are filled up by condensed hydrogen gas; and naturally, when the hydrogen or mercury is driven out of the molecular interspaces, movement of the molecules on one another is less impeded, and hence the former toughness or elasticity is restored.

Nor is amalgamated zinc the only analogous case; for the following remarkable experiment lends further probability to the theory, by showing how rapidly the absorption of zinc by iron may take place, attended

* By the term alloy I mean a solution of one metal in another.

with similar results, as regards increased brittleness, to those which accompany the absorption of hydrogen. It also shows how rapidly, by a slight change of temperature, zinc may be disengaged from the iron, thereby causing it to regain its former toughness.

A piece of galvanized iron wire, of good quality, such that when cold it could be bent several times on itself and back again before breaking, was raised to a red heat so quickly that the coating of zinc was melted and only a small portion vaporized. On attempting to bend it whilst still red-hot, it broke off sharp, offering very little resistance to fracture. The fracture was of a uniform blue-grey colour, as though the zinc had penetrated into the interior of the iron. When cold, the same piece broke with all its former toughness and with a long fibrous fracture. The wire was again heated till the coating of zinc was completely vaporized, and then it was found to be so tough that it was impossible to break at a red heat. Wire in red-hot molten zinc will often break short, though the part out of the metal remains quite tough.

It is remarkable that this experiment will not succeed with all kinds of iron, some not being made thus "red short" by zinc.

By way of testing the theory that occluded hydrogen is the cause of the change in the properties of iron after its immersion in acids, the writer determined to dispense with acid altogether, and endeavour to produce the same result by subjecting pieces of iron to the action of nascent hydrogen.

"With this view two pieces of iron wire ·07 inch diameter were connected respectively with the zinc and copper plates of a battery of 80 Daniell's cells, and immersed in a vessel of Manchester town's water at a distance of 1 inch apart. On closing the current, bubbles of hydrogen were given off from the wire connected with the zinc plate of the battery, but none from the wire connected with the copper plate, the oxygen liberated there apparently forming oxide of iron, which in 12 hours formed a muddy deposit at the bottom of the vessel. After 24 hours the surface of the wire connected with the zinc plate was unchanged; but on moistening the fracture bubbles were given off, just as if it had been immersed in acid. The other wire, though much oxidized and eaten away, did not give off bubbles when broken, and had not become brittle.

"A variety of experiments made in the same way with pieces of wire varying from 3 to 20 inches long, and immersed 5 to 24 hours in water, yielded similar results. It was found, however, that when the wire connected with the zinc plate was of steel, no bubbles were visible to the naked eye on wetting the fracture with the tongue, precisely as in the case of steel after immersion in acid. Twenty-four hours in a warm room restored the iron to its original state, and no bubbles were then seen on breaking and moistening the fracture.

"The water in the last experiments was then replaced by an aqueous solution of caustic soda, when, after two hours, the moistened fracture of

the wire connected with the zinc pole of the battery was found to bubble. Twenty-two hours' longer immersion, the battery working all the time, caused the bubbles to be more abundant; the toughness of the wire was also diminished and its surface blackened. The wire at the positive pole was, however, unchanged, either on the surface or in toughness "*.

From this we see that not only is acid not indispensable for the production of, at all events, the major portion of these changes in iron, but the latter can be equally well produced in an alkaline solution.

The apparatus remaining unchanged, the soda was next replaced by hydrochloric acid, 1·20 sp. gr. On then immersing the iron-wire electrodes for only 2 or 3 seconds, the negative electrode, where hydrogen was given off, was found to froth freely when the fractured extremity was wetted, as much, in fact, as after 15 minutes' immersion when the current was broken. Half an hour's immersion failed to produce any similar change on the positive electrode where no hydrogen was liberated. The absence of effect on the positive electrode is all the more remarkable, as a piece of wire of exactly the same quality, and immersed an equal time in the same liquid, but unconnected with the battery, had become brittle and frothed when broken. It thus appears that neither oxygen nor chlorine are, under these conditions, occluded by iron, or if occluded, that they produce no sensible change in its physical properties.

Nascent hydrogen having been shown to produce these effects, a trial was next made to ascertain if any similar change could be produced in iron by leaving it in an atmosphere of hydrogen gas. With this object a glass tube $\frac{1}{2}''$ in diameter was filled with pieces of bright iron wire $\frac{1}{10}''$ in diameter, and a current of hydrogen passed through for periods of 1, 2, and 8 hours respectively, but without any perceptible change in the wire. The wires were then placed in a bottle three fourths full of water, and hydrogen made to bubble violently through the water for an hour, but still without any effect. It would thus appear that hydrogen is only occluded in the nascent state by iron in the cold. Possibly, however, absorption may take place if the surfaces are chemically clean. The late Dr. Graham, in his valuable papers on the occlusion of hydrogen, showed, several years ago, that when red-hot iron, palladium, or platinum are allowed to cool in an atmosphere of hydrogen, this gas is occluded by them in large quantity; and in the 'Proceedings of the Royal Society,' 1868, xvi. p. 422, he mentions that the best way of charging any of these metals with hydrogen is to make the metal act as the negative electrode in acidulated water for a battery of 6 Bunsen's cells—a fact unknown to the writer when he made experiments.

Though the absorption of hydrogen by iron is no doubt the cause of the frothing and diminution of toughness attendant on the immersion of iron in hydrochloric and sulphuric acids, there are some phenomena which

* Proc. Lit. and Phil. Soc. Manch. 1874, p. 130.

cannot be explained by it alone, but which seem to show that the occlusion of hydrogen is accompanied by the absorption of a minute portion of the acid by the pores of the iron.

In proof of this the following well-established facts are adduced :—

1st. Iron much sooner regains its natural state after immersion in hydrochloric than in sulphuric acid, though at first both may have equally affected it, as judged by diminution of toughness. It may be thought that some portion of the less volatile sulphuric acid adhering to the surface of the iron, even after prolonged washing, will account for it. This cannot be the case however; for the wire may not only be repeatedly washed in water, but even coated with lime-water, dried, and finally reduced in diameter two thirds by drawing several times through a steel die, processes which must surely remove any surface-coating of acid; and yet it will take longer to recover its original toughness if cleaned in sulphuric acid than in hydrochloric acid.

2nd. The pieces in the last experiment immersed in hydrochloric acid will become spotted with rust on the surface some days before those immersed in sulphuric acid.

If the supposition that acid is absorbed by the iron be correct, this is only what we should expect; for it is only natural to suppose that the most volatile acid, viz. hydrochloric, will come to the surface first.

We know, moreover, that water can by great pressure be forced through considerable thicknesses of cast iron. Why, then, should not a liquid pass into the pores of wrought iron?

As further proof of the presence of acid in iron, I have found that blue litmus-paper was slightly reddened when moistened by a drop of water which had been carefully placed on the fracture of a piece of iron ·45″ in diameter, previously immersed in sulphuric acid several hours, and then washed in water. The drop of water in this case did not moisten the sides of the fracture, where some trace of acid might be present.

The occlusion of hydrogen by iron when immersed in acid solutions enables us satisfactorily to account for some of the difficulties experienced in depositing copper, silver, tin, or other metals from their solutions in acid in electrotyping or otherwise. Generally any coat of appreciable thickness slowly shells off, leaving the surface of the iron bare in places, and so making the coating of no avail as a preventive of oxidation. The cause is obvious; the hydrogen occluded whilst the iron is being cleaned in a bath of vitriol or hydrochloric acid, and subsequently imprisoned by the coat of metal, must escape, and in so doing forces its way out, loosening or carrying away some portion of the superficial covering of the metal. If, however, the iron, after being cleaned in acid, is boiled in caustic-soda solution, a process which effectually expels the occluded hydrogen, a coating of copper or other metal may then be electrically deposited which will not shell off in the least. This is actually being done in practice; and

large numbers of iron articles are now coated with a covering of copper four thousandths of an inch in thickness.

In connexion with this subject, I wish to refute a statement made by Professor Reynolds, in a paper read before the Lit. and Phil. Society of Manchester, Feb. 24th, 1874, an abstract of which appeared in the 'Journal of the Chemical Society,' June 1874, p. 546, and other journals. The Professor states that I did not attribute the frothing of iron after immersion in acid to the escape of hydrogen, but to the action of acid. In my first paper on this subject (Proc. Lit. and Phil. Soc. Manchester, p. 80, 1873) the following passage occurs:—"It seems probable that a part of the hydrogen produced by the action of the acid on the iron may be absorbed by the iron, its nascent state facilitating this. And when the iron is heated, by the effort of breaking it, the gas may bubble up through the moisture on the fracture." This shows that in my first paper I comprehended the true nature of the phenomenon.

Change produced in the breaking-strain and ultimate elongation of iron and steel by hydrogen occluded in it after immersion in hydrochloric and sulphuric acids.

In the earlier part of this paper some few of the changes in the properties of iron produced by occluded hydrogen have been examined. The degree of this change it has not always been possible to determine. In the case of the diminution of toughness, for example, no exact and easily applied test has yet been devised by which we can obtain with precision a numerical result expressing the relative toughness of any two samples; consequently we must be content with less definite results. This difficulty is fortunately not met with in the examination of the change in elasticity and tensile strength; for the breaking-weight and maximum elongation of any number of samples can be pretty easily ascertained, with great accuracy, and numerically expressed, thus making comparison easy.

Bearing in mind the numerous uses of iron and steel, and the probability that at times hydrogen may be occluded in them, altering their strength in a way little anticipated, it seemed of some importance to determine these changes—and the more so, as any experiments of this kind could not fail to throw some light on the molecular arrangement of the metal in different qualities of iron and steel, a subject in itself of much interest. With this object upwards of 350 experiments have been made at various times with a very accurate machine, by which any weight could be brought to bear on the wire to be tested without the least jar—a very important point, though difficult of attainment, in experiments on tensile strength. The elongation at any moment could also be easily read off. The length of the pieces tested was in all cases the same, viz. 10 inches between the dies, and the temperature at the time of experiment about 16° C. I mention these points, as any variation in the length or tempe-

rature of the pieces tested will alter the result considerably. In order also to obviate, as far as possible, errors arising from the irregularity and absence of perfect homogeneousness of structure, even in the most carefully prepared iron and steel, the number of tests has been multiplied as much as possible and the mean only given.]

The mode of experiment was as follows:—After immersion in acid, the pieces were wiped and then tested, this giving the tensile strain and elasticity when containing occluded hydrogen; subsequently they were heated on hot plates or in ovens, as the most ready method of expelling the hydrogen, to a temperature considerably below that required to anneal them; and when cold, the breaking-strain &c. of the iron, which had now recovered its natural state, was again ascertained by testing.

It might be thought that tests of iron in its natural state could be best made by experimenting on it before immersion in acid. Results so obtained cannot, however, be fairly compared with tests of the same piece made after immersion in acid, as the action of the acid somewhat reduces the diameter of the iron.

The following results are the means of 30 tests made on *annealed* and *bright* iron wire respectively—first, after being one hour in hydrochloric acid, and, secondly, after being heated 12 to 48 hours to drive off the hydrogen.

	Breaking-strain.	Mean error in breaking-strain.	Elongation.	Mean error in elongation.	Number of experiments of which each result is mean.
		per cent.	per cent.	per cent.	
Annealed iron wire when containing H...	100	±1·16	20·5	±1·12	12
Annealed iron wire, H expelled	100·487	±1·37	21·3	±1·71	12
Bright iron wire when containing H	100	±2·57	2	±0·66	3
Bright iron wire, H expelled	100·274	± ·47	2·83	±0·64	3

Thus the tensile strain of *annealed* iron wire appears to be affected to twice the extent that *bright* wire is by immersion in acid for same length of time. The reverse is the effect on the elongation.

Longer immersion in acid causes the iron to take up more hydrogen, and makes the change much greater, as the following experiments show. Denoting by 100 the breaking-strain of *bright* charcoal-iron wire after 12 hours' immersion in very dilute sulphuric or hydrochloric acid, and subsequent 5 hours' exposure in air at a temperature of 12°, during which time some of the occluded hydrogen must have escaped, then the break-

ing-strain of the same, after being 5 days on a hot plate to expel the hydrogen, is as follows :—

	Breaking-strain.	Mean error in breaking-strain.	Elongation of length tested.	Mean error in elongation.	Number of experiments for each result given.
		per cent.	per cent.	per cent.	
Charcoal-iron wire containing H occluded in H^2SO^4	100	±1·33	1·3	±0·23	6
Charcoal-iron wire, H expelled by heat	106·62	±7·10	4	±0·33	6
Charcoal-iron wire containing H occluded in H Cl	100	±0·3	1·41	±0·41	6
Charcoal-iron wire, H expelled by heat	105·35	±1·1	4·6	±0·33	6

The diminution of elongation and breaking-strain caused by occlusion of hydrogen is very marked in these experiments, but is quite equalled by the following experiments on mild steel containing about 0·227 per cent. carbon. The wires were allowed to remain in very dilute hydrochloric acid about 5 hours, then, when tested, heated to about 100° C. for 12 hours, by which means a portion of the occluded H was expelled.

	Breaking-strain.	Mean error in breaking-strain.	Elongation.	Mean error in elongation.	Number of experiments for each result given.
		per cent.	per cent.	per cent.	
Annealed mild steel containing H	100	± 5·08	20·1	±0·61	9
Annealed mild steel, H partially expelled by heat	104·77	± 3·81	15·4	±1·2	9
Bright mild steel before immersion in H Cl	104·03	±10·1	1·66	±0·44	6
Bright mild steel containing H	100	± 9·2	2·8	±0·46	6
Bright mild steel H partially expelled by heating 12 hours	108·68	± 1·5	2·16	±0·38	6
Bright mild steel, H completely expelled by heating 7 days	114·29	± 8·4	3·42	±0·75	6

These experiments show :—

1st. That the tensile strain of steel is diminished by the occlusion of hydrogen, and that as the hydrogen is expelled (a process of long duration)

the tensile strain rises, till eventually it exceeds the original strain before immersion in acid.

2. A most unexpected change in the elasticity of steel, the elasticity of the wire being considerably increased by the occlusion of hydrogen; but when a portion of this hydrogen is expelled by heat, the elasticity, as measured by elongation at the moment of fracture, falls remarkably, as much as 4·7 per cent. in annealed and 0·64 per cent. in bright steel. When the hydrogen is completely expelled, the elasticity, however, rises, being then greater than before immersion in acid.

The following experiments on hardened and tempered cast-steel wire containing about three times as much carbon as the mild steel, show an extraordinary diminution in the tensile strain when containing occluded hydrogen; this, however, is regained or even surpassed when the hydrogen is expelled by heat.

	Breaking-strain.	Mean error in breaking-strain.	Elongation.	Mean error in elongation.	Number of experiments for each result given.
		per cent.	per cent.	per cent.	
Steel before immersion in acid..................	123·79	±2·7	2·16	±0·27	6
Steel immersed in H^2SO^4 12 hours......	100	±4·9	1·916	±0·416	6
Steel, H expelled by heating 10 days to from 100° to 200°...	122·53	±6·09	2·66	±0·55	6
Steel before immersion in H^2SO^4	100·27	±1·28	3·75	±0·25	2
Steel after immersion in H^2SO^4 1 hour ...	100	±2·19	3·75	±0·25	2
Steel heated 24 hours to 100°–200°, to expel H	105·49	±2·9	2·75	±0·25	2

The change produced by occluded hydrogen must have an important influence on the stability of all iron and steel structures; for as the rusting of iron is mainly attributable to the action of the carbonic acid in the air, it is probable that the hydrogen liberated when the acid attacks the metal is occluded by the iron or steel, with consequent diminution of tensile strength and elasticity. In some cases, where rust has spread very rapidly, the writer has noticed a decided diminution of toughness; but, as a rule, it is difficult to detect any change, as probably the hydrogen is present in very small quantity; also when it has reached a certain percentage its tendency to escape from the metal will balance the force of occlusion.

Electric Conductivity.

Several experiments have been made to ascertain if there is any alteration in the electric conductivity of iron wire when containing hydrogen.

Professor Stewart kindly allowed me the use of the Owens College apparatus, with which some of the following results were obtained:—

Resistance of 6 feet bright charcoal-iron wire = 100	100
Resistance of 6 feet bright charcoal-iron wire after 5 hours in dilute H^2SO^4 = 107·14, or, allowing for iron eaten away by acid....................................	105·6
Resistance of 6 feet bright charcoal-iron wire after 5 hours in dilute H Cl = 114·3, or, allowing for iron eaten away by acid	109·4

The wires were somewhat eaten away by the acid, so allowance had to be made for the increased resistance due to decreased sectional area; this is made in the column to the right.

About 50 feet of hard bright iron wire, after 24 hours' immersion in dilute sulphuric acid, gave a resistance of 2·94 ohms, and 2·92 ohms after the occluded hydrogen had been expelled by heat.

The above results, though far from uniform, are sufficient to show that there is an increase in the resistance of iron wire when it contains occluded hydrogen. I hope soon to make further experiments on this subject. It is worthy of remark that Professor Graham found the resistance of palladium containing occluded hydrogen was increased about 25 per cent. He also discovered that a palladium wire first elongated when charged with hydrogen, and then contracted when the hydrogen was withdrawn to less than its original length. The writer has detected a very small and similar change in the length of annealed iron wire under like condition, but has not yet observed it in bright iron wire, though he does not despair of doing so.

Diffusion of Hydrogen.

A number of experiments were made by allowing one half of a piece of bright iron or steel wire to be acted on by dilute acid, and thus to occlude hydrogen while the other half was protected from this action, with a view of ascertaining if the occluded hydrogen could spread along the interior of the iron. Great difference was observed in the behaviour of iron and steel; the fibrous structure of iron wire allows the hydrogen occluded in the part acted on by acid to spread into the other part, distinct traces of hydrogen being observed 17 centims. from the part affected by acid. The close unfibrous structure of steel, on the contrary, seems to oppose this altogether, it being questionable if the hydrogen spreads 2 to 3 centims. beyond the part immersed in acid.

When that part of the iron wire which was protected from but still affected by the acid was broken and the fracture moistened, the bubbles of gas arose almost exclusively from the centre of the fracture, while from the part immersed in acid they arose equally from the whole surface, and took less time to attain their maximum.

January 21, 1875.

The Right Hon. LYON PLAYFAIR, C.B., LL.D., Vice-President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On the Origin and Mechanism of Production of the Prismatic (or Columnar) Structure of Basalt." By Robert Mallet, F.R.S. Received December 12, 1874.

(Abstract.)

The author having briefly traced the history of geological opinion on this subject, from before the period at which the controversy at which the igneous or aqueous origin of basalt might be viewed as settled, and having stated the views of some of the more prominent British authors on this subject of recent date, points out that, up to the present, no clear and definite theoretic views have been enunciated to account for prismatic structure in basalt, and that it is impossible to gather, with any distinctness, from systematic writers, whether prismatic structure be due to contraction by cooling alone, or whether the structure is due to preexisting concretionary or crystalline arrangement of the integral particles of the mass, or to this coacting with enormous external pressures, the origin of which is left perfectly vague, or to some play of successive and joint actions of all these various forces.

Prof. James Thomson, in a paper read some years ago, and since repeated at the late Belfast Meeting of the British Association, has proposed views, in some respects new, tracing prismatic structure to contraction by cooling only, and has expressed entire doubt as to the part supposed to be played by concretionary spheroids pressed together. Prof. Thomson's views, however, are still far from complete, and the mode assigned by him to the production of cup-shaped cross joints in the prisms fails to account for the phenomena.

The aim of the author is to point out in this paper that all the salient phenomena of the prismatic and jointed structure of basalt, as observable in nature, can be accounted for upon the admitted laws of cooling, and contraction thereby, of melted rock possessing the known properties of basalt, the essential conditions being a very general homogeneity in the mass cooling, and that the cooling shall take place slowly, principally from one or more of its surfaces.

Thus, taking the simple case of a tabular mass of molten basalt, whose top surface is level, the depth being great and the other two dimensions

indefinitely greater than that, and assuming the material at one temperature initially, homogeneous and isotropic, and that cooling takes place from the top surface only, he, on these data, proceeds to consider the phenomena that will successively result by contraction in cooling.

While the mass remains at its upper part still plastic by heat, contraction will be met by internal movements and subsidence of the top surface, and no cracking or splitting can take place until the material there has become rigid enough to break under tensile strain. He points out that this degree of rigidity, or "splitting temperature," is not reached until the top surface has fallen to between 900° and 600° Fahr.

At this temperature the cooling surface begins to separate, by fracture penetrating perpendicularly to it, into smaller surfaces. These must be similar and equal in area, and such as that their edges in contact can make up a continuous superficies. To relieve the orthogonal strains in the cooling surface, and to meet the above conditions, only three geometric figures for the separating surfaces are possible—namely, the equilateral triangle, the square, and the regular hexagon.

The author then inquires why the last of these is normally the form found in nature. He traces this to the law of least action which governs the play of all natural forces whose final result is produced by the least possible expenditure of force. He shows that, in a contracting surface splitting up into equal areas, the expenditure of work will, for the equilateral triangle, the square, and the regular hexagon, be approximately as the numbers 1·000, 0·680, and 0·519. This economy of force decides the hexagon as the form found in nature. The diameter of the hexagon, which is the upper surface of the inceptive hexagonal prism, is shown to be fixed by the relation that subsists between the coefficient of contraction of the material and that of its extension at rupture by a tensile force at the splitting temperature. This decides the diameters of the separate prisms. The splitting by contraction proceeds into the mass always in a direction perpendicular to the cooling surface; and at any instant the splitting is limited in its progress by the isothermal *couche* which is at the splitting temperature within the mass; for within that *couche* the mass is still plastic. In the case assumed, the prisms formed are straight and vertical. When the splitting has proceeded to some distance within the mass, the further cooling of each prism takes place, not only from the top, but from the sides; and the more important conditions influencing the latter in nature are pointed out.

Any one prism is coldest at its extremity, and its temperature increases along its length to the other end, where the splitting is still proceeding. The prism is hotter also, for any transverse section, as we approach its axis than about the exterior; differential strains in the longitudinal direction thus take place, by cooling and contraction, between the successive imaginary *couches*, taken from the exterior to the axis of the prism,

which tend to cause the outer portions of the prisms to tear asunder at intervals in length dependent, like the diameter of the prisms themselves, upon the relation subsisting between the coefficient of contraction and of extensibility at rupture of the material.

The prism contracts not only in its length, but in its diameter; transverse fracture at its surface, when it occurs, is therefore due to the resultant of two orthogonal forces, the one parallel to the axis of the prism, as already referred to, and the other in a plane transverse to the axis. These two forces are proportionate, the first to the length of the prism from a preceding joint or from its extremity, the second approximately to the semidiameter of the hexagon or mean radius of the *couche*; and the resultant of these two, at any point taken round the prism, is oblique to the axis and tending towards it in direction. As fracture in a homogeneous solid always takes place transverse to the line of strain, so the fracture producing a transverse joint takes place oblique to the sides of the prism—the obliquity becoming less as the fracture advances towards the axis of the prism, so that when complete it is cup-shaped, the convex surface of the fracture always pointing in the same direction as that in which the splitting of the prism itself is proceeding.

This solution, which is believed to be the first ever presented which, resting upon admitted laws, completely accounts for the production of the very remarkable cup-shaped joints, is verified and illustrated by several diagrams, showing the mode of production of these joints and the modifications of their curvature produced by varied conditions in the cooling.

It is further shown that the partial or complete detachment of certain fragments, frequently observed to be partially or wholly detached from the cusps of the concave side of these joints at and near the solid angles of the hexagon, is a consequence necessarily resulting from the mode of production of the joints themselves. The author then points out that, in the case of very slender prisms, other (and mechanical) conditions besides those of differential cooling enter into the production of the cross joints, which are at more considerable and irregular distances apart, and in planes of fracture often nearly transverse to the axis of the prism.

He also discusses the modifications produced in the prisms themselves, and in their cross joints, by heterogeneity in the mass of basalt itself—as, for example, by a more or less previously developed cleavage in the basalt in planes transverse to the axis of the prism, or by the presence of heterogeneous substances imbedded in the mass. To these latter, and to differences in conductivity or in the cooling energy at different points of the cooling surface, are chiefly to be ascribed the divergences from the normal hexagonal form of the prisms as occasionally observed, the author remarking that where such divergences occur they disappear, and the

normal hexagonal form is returned to in such a manner as to require only the minimum expenditure of work.

The conditions producing greater or less interspaces between the prisms, which may vary from point to point of the same mass, are pointed out, as also those which cause the spaces between successive joints in adjacent prisms to coincide in successive planes, transverse to their axes or the contrary.

The author then proceeds to discuss the various positions in space, and relatively to each other, which the axes of the prisms must assume, dependent on the general law, as already stated, that the axes of the prisms, however produced, are always normal to successive isothermal *couches* or planes at the splitting temperature, taken in succession within the mass.

If the mass be tabular, as already assumed, and cooling take place only from the top surface, the prisms will be straight and vertical, extending from top to bottom nearly of the tabular mass, and being separated from the bottom on which it rests by a more or less thick layer of irregular angular fragments, or of badly conducting material, tufa, scoriæ, &c., the convex surfaces of the cross joints all pointing downwards. If the mass cool both from the bottom and the top, the prisms, vertical and straight, will split upwards and downwards, and meet in an irregular intermediate stratum of angular fragments, the convex surfaces of the joints of the lower prisms pointing upwards, and the respective lengths of the upper and lower ranges depending on their relative rates of cooling. If the tabular mass cool also from one or more of its sides, as by an abutting wall of rock, prisms will be produced with their axes perpendicular to that wall, and will be separated from the vertical ranges of prisms by an inclined stratum of angular fragments. Also, if the basalt fill a crevasse producing a dyke, the prisms formed by cooling will be generally transverse to the plane of its walls, and meet somewhere towards the centre in a stratum of more or less irregular fragments, due in all cases to the irregular contractions at the extremities of the prisms breaking up their mass there into wholly irregular forms. If the upper and cooling surface have a curved convex contour, the prisms will be taper and convergent from the surface of the mass; and, on the contrary, if the cooling surface have a concave contour, or rest upon a concave bottom, the prisms will be divergent from the interior of the mass, the natural law of economy of work limiting the length or amount of taper in either case and the length of the prisms, and at a certain length of prism a new range of larger diameter partially or wholly then commencing. The convergence or divergence are simple consequences of the general law, that the splitting takes place always normal to the isothermal *couches* which are at the splitting temperature.

The author then proceeds to develop and illustrate by diagrams some of the varied and curious combinations which are observable in nature,

and due to the more or less combined play of these conditions. He then proceeds to develop, as a consequence of the general law, the production of curved prisms, or those with apparently bent axes, which are observed in almost all basaltic countries. If the cooling mass of basalt be in one of its vertical sections of such a form that successive isothermal *couches*, taken in descending order, are not parallel to the original cooling surface, as they are in all cases of straight and parallel prisms, but divergent gradually from the cooling surface and from each other, then the lines of splitting of the prisms, always normal to these *couches*, must be curved in one direction. This will be true whether the isothermal *couches* be plane surfaces divergent from a thinner to a thicker part of the mass, or whether they be curved surfaces arising from the mass reposing on a curved bottom and diverging in like manner. This explanation of the production of curved prisms, without the necessary intervention of external mechanical forces having bent into curves prisms originally formed straight, is, the author believes, here for the first time presented. He shows that great difficulties exist to the supposition that curved prisms are ever the result of the bending of prisms originally straight by extraneous mechanical effort. The author having thus shown that all the salient phenomena presented in nature by the forms, jointings, positions of the prisms, &c. of columnar basalt are accounted for as consequences of contraction in cooling, submits that this solution given by him must be the true one. He, however, proceeds to examine at some length the different views of those who have imagined that prismatic and jointed basalt has resulted from the squeezing together, by some wholly imaginary external force, of spheroidal masses more or less resembling those known as "onion stones," or so-called concretionary spheroids, such as those imagined by Mr. Gregory Watt. The author submits all points of the subject to a searching examination, and points out that, upon the only probable suppositions that can be made as to the prearrangement of such spheroids, no extraneous force of compression could produce prisms at all, but must squeeze the spheroids instead into rhombic dodecahedrons.

II. "On the Anatomy of the Connective Tissues." By G. Thin, M.D. Communicated by Prof. Huxley, Sec. R.S. Received December 23, 1874.

(Abstract.)

Transparent animal tissues, when sealed up fresh in aqueous humour or blood-serum, by running Brunswick black round the edge of the cover-glass, undergo a series of slow changes, by which, generally within a period of 2 to 5 days, anatomical elements mostly otherwise invisible become

distinct. The paper is chiefly a record of observations made by this method. The author shows :—

1. After a horizontal section of the cornea has been sealed up for about 24 hours, the stellate branched cells are seen to consist of a mass of protoplasma, sharply defined on every side, except where it is continued for a scarcely perceptible distance into the processes. The nucleus is flattened. The processes become very fine, glistening, and thread-like almost immediately after leaving the cell, and, by dividing and anastomosing with the processes of other cells, form a rich and very delicate network.

2. It sometimes happens, although only in rare instances, that, in gold preparations, fine dark lines extend between the nuclei, and correspond in outline and course with the processes seen in the aqueous humour; and it is then evident that they are surrounded by the dark-coloured tracts which form the ordinary network seen in gold preparations, and which correspond, in outline and varying degree of development in different animals, ages, and pathological conditions, with the corneal spaces.

3. Similar appearances to those described in paragraph 1 are seen in sections of cornea which have been 5 to 10 days sealed up in a 10-per-cent. solution of common salt.

4. The quadrangular and long narrow flat cells shown by the author to exist in the cornea, by means of a saturated solution of potash, are also rendered visible by the above method. They are best seen in oblique sections, from which, after 2 to 5 days, they fall out singly and in rows. A row of the long narrow cells is often seen to terminate in quadrangular cells at either end. These cells have a perfectly hyaline appearance; their nucleus has a very faint yellowish tinge, and projects beyond the surface of the cell.

In exceptional instances, in the uncut cornea of the frog, the long flat cells may be seen, after several days' maceration, lying on the primary bundles.

5. In tendon, flat masses of cells are found, on the third to fifth day, lying on the edge of the preparation and free in the fluid. The cells are accurately fitted to each other, after the manner of an epithelium. In the tendo Achillis of the frog they are seen of three sizes :—(*a*) large cells, corresponding to the flat cells seen on the surface by nitrate of silver; (*b*) smaller quadrangular cells, similar in size to those described by Ranvier, and which have been described by the author as investing the secondary and tertiary bundles in double layers; and (*c*) long, narrow, flat cells, similar to those described by the author as being isolable by potash, and as covering the primary bundles.

The masses of the cells of the surface, and of the secondary and tertiary bundles, can be usually seen to consist of a double layer separated by a very thin transparent medium.

6. The perimysium and neurilemma are respectively represented by a

double layer of quadrangular and hexagonal cells, identical in general appearance with an epithelium. Between the two layers there is a thin transparent medium.

7. From the neurilemma of the sciatic nerve of the frog, when cut in narrow longitudinal strips, after a few hours, branched cells of different types of form are seen isolated in the fluid near the cut edges. These cells are of two well-marked general types. In one a small smooth-contoured elongated mass of protoplasma is continuous at both ends with a fine long thread-like fibre; in another an irregularly contoured, but generally somewhat elongated, mass gives off numerous sharply defined, very fine glistening fibres in all directions. Sometimes a protoplasmic centre terminates at one end by a single fibre, and by two at the other. These fibres are often of great length, and the protoplasmic mass can sometimes only be found by carefully tracing them whilst moving the object-glass.

8. Fibrillary tissue is seen to be composed of uniform flat, ribbon-like bands, whose breadth approaches the diameter of a human red blood-corpuscle. These are seen in their simplest form when extruded from the neurilemma of the sciatic nerve of the frog, which takes place within 24 hours' maceration. From their position in this membrane they form part of the transparent medium which exists between the two layers of quadrangular cells. They are mostly marked by a puckered appearance transversely.

In skin and tendon, after a few days' maceration in the sealed fluid, the fibrillary tissue is seen to be composed of extremely fine but sharply contoured fibrillæ, arranged in parallel bands, which are of the same breadth as the soft ribbon-like bands which are isolable from the neurilemma.

The respective appearances in the neurilemma and in tendon indicate extremes in the condition of this tissue, and represent, according to the author, primary bundles of connective tissue.

9. The primary bundles of the cornea are seen only exceptionally by this method, but can be demonstrated with great precision by sealing up a frog's cornea in a mixture of equal parts of half-per-cent. solution of chloride of gold and concentrated acetic acid.

10. In nerve-bundles, after 24 hours' maceration in aqueous humour, some of the medullated fibres may be seen to have their contour broken transversely by straight hyaline spaces. The author assigns this appearance to the peculiarity of structure described by Ranvier.

11. The breadth and appearance of the rods of the frog's retina are nearly identical with those of the primary bundles of the neurilemma.

The transverse markings described by Max Schultze as being produced by the action of osmic acid on the rods and cones, resemble the transverse puckerings in the primary bundles. In both rods and primary bundles, after prolonged maceration in aqueous humour, the free ends of each indi-

vidual element bend in one direction until they join, and the substance of the ring thus formed undergoes in both a similar and peculiar process of disintegration. From these facts the author infers that the rods and cones of the retina are composed of fibrillary tissue in its simplest form.

12. Transverse sections of muscular fibre, when examined at intervals, show varying appearances, only a small minority of such preparations being successful. Successful preparations show one or more of three appearances :—(*a*) primary bundles, corresponding to Cohnheim's fields; (*b*) groups of these (secondary bundles), the aggregate of which fill up the space bounded by the sarcolemma; (*c*) a threadwork of fine fibres surrounding the primary bundles, in meshes.

13. Examination of connective tissue, in various stages of inflammation, yields strongly confirmatory evidence in favour of the interpretation given by the author to the appearances above described.

January 28, 1875.

JOSEPH DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On the Theory of Ventilation: an attempt to establish a positive basis for the calculation of the amount of Fresh Air required for an Inhabited Air-space." By Surgeon-Major F. DE CHAUMONT, M.D., Assistant Professor of Hygiene, Army Medical School. Communicated by Prof. PARKES, M.D., F.R.S. Received November 18, 1874.

The question of ventilation, and the amount of fresh air required to keep an inhabited air-space in a sweet and healthy condition, has been much discussed of late years, and very fully treated of by various writers; but there was a good deal of vagueness and want of precision in the manner of treatment previous to the Report of the Committee on Metropolitan Workhouse Infirmaries in 1867. In a paper in the 'Lancet' in 1866 I attempted to show that a more scientific method might be employed, and suggested some formulæ, which were quoted by Dr. Parkes in a paper appended to the Report above mentioned. Professor Donkin also investigated the question there, and in a short but exhaustive paper showed

that, general diffusion in an air-space being admitted, the same amount of air was required to ventilate it, whatever its size might be. In another paper, published in the 'Edinburgh Medical Journal' in May 1867, I went into the subject with the view of pointing out that we might, with existing data, establish a basis, which should be both scientific and practical, for estimating the amount of air required; and I adduced some results to show that the evidence of the senses might be employed (if used with proper care and precautions) as the ground-work of a scale, and gave a short table of the amounts of respiratory impurity (estimated as CO_2) which corresponded to certain conditions noted as affecting the sense of smell. This paper attracted the attention of General Morin, who made it the text of a short article in the Journal of the Conservatoire des Arts et Métiers during last year. Since the publication of my paper in 1867 I have accumulated more data; and the number of observations being now sufficient to give at least a fair approximation to the truth, I beg to call attention to the results.

It is generally admitted that it is organic matter, either suspended or in the form of vapour, that is the poison in air rendered impure by the products of respiration. It is also admitted that it is the same substance that gives the disagreeable sensation described as "closeness" in an ill-ventilated air-space. Although the nature of the organic matter may vary to a certain extent, it will be allowed that a condition of good ventilation may be established if we dilute the air sufficiently with fresh air, so that the amount of organic matter shall not vary *sensibly* from that of the external air. Unfortunately all the methods devised for the determination of organic matter in air are both difficult and unsatisfactory, so much so that they are almost practically impossible in a ventilation inquiry. Observations, however, as far as they have gone, seem to show that the amount of organic impurity bears a fairly regular proportion to the amount of carbonic acid evolved by the inhabitant in an air-space; and as the latter can be easily and certainly determined, we may take it as a *measure* of the condition of the air-space. This being accepted, and general diffusion being admitted, we can easily calculate the amount of fresh air required to bring down the CO_2 to some fixed *standard*, adopting as a datum the ascertained average amount of CO_2 evolved by an adult in a given time. If, now, we adopt as our *standard* the point at which there is no sensible difference between the air of an inhabited space and the external air, and agree that this shall be determined by the effects on the sense of *smell*, our next step is to *ascertain* from experiment what is the average amount of CO_2 in such an air-space, from which we can then calculate the amount of air required to keep it in that condition. The sense of smell is very quickly dulled, so that, in order to keep it acute, each air-space to be examined ought to be entered directly from the open air. For this reason I have not included in the present paper any of the observations made in prisons, as it is almost impossible, from their con-

struction, to enter the cells directly from the open air. All the results, therefore, have been obtained in buildings where this could be done, viz. barracks and hospitals, and the following are those examined :—

Aldershot Permanent Barracks.
Hilsea Barrack Huts.
Hilsea Hospital (Pavilion building).
Herbert Hospital (Pavilion), Woolwich.
Chelsea New Barracks.
Tower of London.
Gosport New Barracks.
Anglesea Barracks, Portsea.
Fort Elson } casemates.
Fort Brockhurst }
Garrison Hospital, Portsmouth.
Civil Infirmary, Landport.

The plan followed in all was to take the observations chiefly at night, when the rooms or wards were occupied, and when fires and lights (except the lamp or candle used for the observation) were out. In this way all disturbing sources of CO_2 were avoided, except in the occasional rare instances of a man smoking in bed or the like. On first entering the room from the outer air the sensation was noted and recorded just as it occurred to the observer, such terms as "fresh," "fair," "not close," "close," "very close," "extremely close," &c. being employed *. Most of these notes were made by myself; but a good many were also made by my assistants, Sergeant (now Lieutenant) Sylvester in the earlier, and Sergeant H. Turner in the latter experiments. The air was then collected (generally in two jars or bottles, for controlling experiments), and set aside with lime-water for subsequent analysis, and the temperatures of the wet- and dry-bulb thermometers noted. About the same time samples of the external air were also taken, and the thermometers read. In this way any unintentional bias in the record of sensations was avoided, and this source of fallacy fairly well eliminated.

In some of the earlier observations the CO_2 in the external air was not observed as constantly in connexion with the internal observations, partly because the importance of this was not so clearly perceived then, and partly from want of apparatus, the jars used being very bulky and not easy of carriage. It might therefore be argued that the *combination-weights* of the earlier experiments should be less in calculating the averages. I do not think, however, that this would amount to any sensible difference in the result, as the external CO_2 ratios adopted from single experiments accord fairly with the mean ratio of the outer air †. In each

* N.B. The terms used in the Tables are *exactly* those noted down at the time of observation.

† Mean ratio of the whole series ·372; omitting those at Portsmouth Garrison Hospital, which were exceptionally low, 413.

case the CO_2 has been corrected for *temperature*, but not *for barometric pressure*, as in some cases the reading of the barometer was not taken; the difference, however, would not exceed on an average 1 per cent. The vapour and humidity were calculated from Glaisher's Tables.

Although the records of sensation are various in terms, I have thought that they might be advantageously reduced to *five* orders or classes, as follows :—

No. 1. Including such expressions as "fresh," "fair," "not close," "no unpleasant smell," &c., indicating a condition giving no appreciably different sensation from the outer air.

No. 2. Including such expressions as "rather close," "a little close," "not very foul," "a little smell," &c., indicating the point at which organic matter begins to be appreciated by the sense of smell.

No. 3. "Close," indicating the point at which organic matter begins to be decidedly disagreeable to the sense of smell.

No. 4. "Very close," "bad," &c., indicating the point at which organic matter begins to be offensive and oppressive to the senses.

No. 5. "Extremely close," "very bad," &c., indicating the point at which the maximum point of differentiation by the senses is reached.

Where there was a slight smell of tobacco no change in the record was made; but where the smell of tobacco was strong, the observation was generally referred to the next order, both because the presence of the tobacco-smoke indicated slow change of atmosphere, and also because the sense of closeness must have been considerable to make itself felt along with the tobacco. Hence such a remark as "rather close," which properly belongs to No. 2, is referred to No. 3, "close," if accompanied with a strong smell of tobacco.

The total number of observations for the temperature, vapour, and humidity in the inhabited spaces amounts to 247*, and of carbonic-acid analyses to 473. Where the latter are in pairs they are linked by a bracket. In each case the external and internal observations and their differences are given, and the arithmetical means of all are taken. In the differences which represent the quantities due to respiratory impurity, the mean error, error of mean square, and probable error (both of a single measure and of the result) are calculated, and the limits shown between which the range would lie in each case. The values are also given as the reciprocals of the squares of mean error and of probable error of result, and their ratios to No. 1 as unity. The modulus is also calculated from the mean error and error of mean square, and the ratio of the two results

* It has been thought unnecessary to give these in detail as taking up too much space, but the means are given at the end of the Table of Carbonic Acid.

thus obtained shown as another means of estimating the value of the series.

Analyses of the different Orders.

No. 1 (see Table No. 1)*.—" Fresh " &c.: a condition of atmosphere not *sensibly* different from the external air.

1. *Temperature.*—The experiments were made during both winter and summer, so that there is a good deal of variation in the external temperature, and the mean is some degrees above the mean annual temperature of this country (southern part of it), viz. 57°·47. The mean in the inhabited air-spaces is 62°·85, or 5°·38 higher. This is a moderate difference, and shows a good average temperature for dwelling-rooms. The maximum range is 10° (57°·89 to 67°·81), calculated from the error of mean square, the actual extremes being 77° and 53°.

2. *Vapour and Humidity.*—As the external temperature varied considerably, so also did the amount of vapour, the mean being 4·285, equal to about 80 per cent. of humidity. The internal observations showed a mean of 4·629, or 73 per cent. of humidity, being an excess of vapour of 0·344 of a grain, and a lowering of relative humidity equal to 7 per cent.

3. *Carbonic Acid.*—The mean external carbonic acid was 0·4168, a little above the usual amount. The mean in the inhabited air-spaces was 0·5998, or an excess of 0·1830, the mean error being 0·0910. The probable error of a single observation is 0·0831, so that the truth would lie between 0·2661 and 0·0999; whilst the probable error of the result is only 0·0078, the range being between 0·1908 and 0·1752; we are therefore entitled to say that the limit of impurity, imperceptible to the sense of smell, lies at or within 0·2000 volume of CO_2 per 1000 as a mean. From these data, then, we may lay down as conditions of *good* ventilation the following:—

Temperature about 63° Fahrenheit.

Vapour shall not exceed 4·7 grains per cubic foot.

Carbonic acid shall not exceed the amount in the outer air by more than 0·2000 per 1000 volumes.

No. 2 (see Table No. 2).—" Rather close " &c.: a condition of atmosphere in which the organic matter begins to be appreciated by the senses.

1. *Temperature.*—In this series the external temperature (although still above the mean temperature of this climate) was rather lower than in the previous one, viz. 54°·85, whilst the internal observations gave a mean of 62°·85 (the same as in No. 1), or a difference of 8°.

2. *Vapour and Humidity.*—Although the temperature was the same as in No. 1, the amount of vapour in the inhabited air-spaces was greater,

* It has not been thought necessary to publish Tables 1–5, but they are preserved in the Archives of the Society.

both actually and relatively, the excess being 0·687 of a grain and the lowering of humidity being about 7·6 per cent.

3. *Carbonic Acid.*—The mean amount in the outer air was 0·4110 per 1000 volumes, in the inhabited air-spaces 0·8004, or a mean difference (respiratory impurity) of 0·3894. The range for the probable error of result lies between 0·4057 and 0·3731.

We may therefore say that ventilation ceases to be *good* when the following conditions are present:—

Vapour in the air exceeds 4·7 grains per cubic foot.

CO_2 in excess over outer air, ratio reaching 0·4000 per 1000 volumes.

No. 3 (see Table No. 3).—"Close" &c.: the point at which the organic matter begins to be decidedly disagreeable to the senses.

1. *Temperature.*—The temperature in this series was more near the mean of our climate, viz. 51°·28. The mean in the inhabited air-space was 64°·67, or a mean excess of 12°·91.

2. *Vapour and Humidity.*—The vapour in the outer air was 3·837, and in the inhabited air-space 4·909, a mean difference of 1·072 grain per cubic foot. The drying of the air amounted to a lowering of the humidity by 11·56 per cent.

3. *Carbonic Acid.*—The carbonic acid in the outer air was 0·3705 per 1000 volumes, rather below the average. In the inhabited air-spaces it was 1·0027, or a mean difference of 0·6332 due to respiratory impurity, the range for the probable error of result being between 0·6477 and 0·6167.

We may therefore say that ventilation begins to be decidedly *bad* when the following conditions are reached:—

Vapour reaches 4·9 grains per cubic foot.

Carbonic acid in excess over outer air to the amount of 0·6000 per 1000 volumes.

No. 4 (see Table No. 4).—"Very close" &c.: the point at which the organic matter begins to be offensive and oppressive to the senses.

1. *Temperature.*—The mean external temperature was 51°·28, and the internal 65°·15, or a mean difference of 13°·87.

2. *Vapour and Humidity.*—The mean vapour in the outer air was 3·678 grains, and in the inhabited air-spaces 5·078, or a mean difference of 1·400 grain per cubic foot. This corresponds to a lowering of the humidity by 8°·58 per cent.

3. *Carbonic Acid.*—The mean amount in the outer air was 0·3903 per 1000 volumes, pretty near the usual average. In the inhabited air-spaces it was 1·2335, or a mean difference due to respiratory impurity of

0·8432, the range for probable error of result being between 0·8640 and 0·8224.

We may say that ventilation is *very bad* when:—

Vapour reaches 5 grains per cubic foot.

Carbonic acid in excess over outer air reaches 0·8000 per 1000 volumes.

No. 5 (see Table No. 5).—"Extremely close" &c.: the maximum point of differentiation by the senses.

1. *Temperature.*—The temperature in the outer air was 51°·86, and in the inhabited air-spaces 65°·05, giving a mean difference of 13°·19.

2. *Vapour and Humidity.*—The mean vapour in the outer air was 3·875, and in the inhabited air-spaces 5·194, showing an excess of 1·319 grain, corresponding to a lowering of relative humidity of 9·88 per cent.

3. *Carbonic Acid.*—The mean amount in the outer air was 0·4001, or exactly the average amount. In the inhabited air-spaces it was 1·2818, showing an excess due to respiratory impurity of 0·8817 per 1000 volumes, the range for the probable error of result being between 0·9202 and 0·8432.

The extreme point of differentiation by the senses is thus reached when the following conditions are found:—

Vapour 5·100 grains per cubic foot.

Carbonic acid in excess over the amount in the outer air beyond 0·8500 per 1000 volumes.

It will at once be seen that the figures in No. 5 differ but little from those in No. 4, and that the probable *limit of differentiation* by the senses is reached in No. 4. The number of recorded observations in No. 5 is also very few comparatively; and I think it would therefore be better to group the two together, as below.

Nos. 4 and 5 combined, being the probable limit of possible differentiation by the senses.

1. *Temperature.*—In the outer air 51°·43, in the inhabited air-spaces 65°·12, or a mean difference of 13°·69.

2. *Vapour and Humidity.*—The vapour in the outer air was 3·729, inside 5·108, or a mean difference of 1·379 grain, corresponding to a lowering of relative humidity of 8·92 per cent.

3. *Carbonic Acid.*—In the outer air 0·3928, in the inhabited air-spaces 1·2461, or a mean difference due to respiratory impurity of 0·8533, the range for probable error of result being between 0·8717 and 0·8349.

We may therefore, I think, say that when the vapour* reaches 5·100 grains per cubic foot, and the CO_2 in excess 0·8000 volume per 1000, the maximum point of differentiation by the senses is reached.

* It is to be understood that the amounts of vapour stated in these cases are in reference to a mean temperature of about 63° F.

By referring to the Tables it will be seen that there is a regular progression as we pass from one order to another. The following abstract shows this:—

No.	Temperature.		Vapour.		Carbonic acid.	
	In air-space.	Excess over outer air.	In air-space.	Excess over outer air.	In air-space.	Excess over outer air.
1.	62°·85	5°·38	4·629	0·344	0·5999	0·1830
2.	62·85	8·00	4·823	0·687	0·8004	0·3894
3.	64·67	12·91	4·909	1·072	1·0027	0·6322
4.	65·15	13·87	5·078	1·400	1·2335	0·8432
5.	65·05	13·19	5·194	1·319	1·2818	0·8817

The progression is complete in the carbonic acid, although there are slight retrogressions in the temperature and vapour of No. 5. Taking the last two combined, we have

65°·12 13°·69 5·108 1·379 1·2461 0·8533

We have now the progression complete throughout. Adopting *four* orders, then, we shall find the regularity of progression sufficiently noteworthy in the *vapour* and *carbonic acid*, the two products of respiration. It is less regular in the temperature, as might indeed be expected, from the varying condition of the external air.

Table of Differences of Temperature, Vapour, and CO_2.

No.	Temperature.		Vapour.		Carbonic acid.	
	Actual excess over outer air.	Progressive difference.	Actual excess over outer air.	Progressive difference.	Actual excess over outer air.	Progressive difference.
1.	5°·38	°...	0·344	...	0·1830	
2.	8·00	2·62	0·687	0·343	0·3894	0·2064
3.	12·91	4·91	1·072	0·385	0·6322	0·2428
4 & 5 (combined).	13·69	0·78	1·379	0·307	0·8533	0·2211

In each observation there is a culmination at No. 3, and a decline at

the next order. The average rates of progression (including the actual excess in No. 1) are :—

Temperature.	Vapour.	Carbonic acid.
3°·42	0·345	0·2133

Here the amount of vapour is exactly the actual excess in No. 1, and the amount of carbonic acid somewhat in excess; the mean, however, between this amount and the actual recorded excess in No. 1 is as follows :—

Actual excess over outer air in No. 1	0·1830
Mean of progressive increase, as above......	0·2133
Sum ......2)	0·3963
Mean	0·1982

This is sufficiently close to 0·2000 to furnish some additional reason for adopting this latter number as the limit of respiratory impurity admissible in *good ventilation.*

Values of the several series, considered relatively to each other.

The values are important as a guide to the more or less trustworthy character of the series. They have been calculated out in three ways :—

1. As the reciprocal of the square of *mean error.*
2. As the reciprocal of the square of probable error of result.
3. As the ratio between the *modulus* calculated from the *mean error* and the *modulus* calculated from the *error of mean square of a single measure.*

The following Table gives the values from the first method, viz. as reciprocal of the square of mean error :—

No.	Temperature.	Vapour.	Humidity.	Carbonic acid.
1.	0·0821	6·1300	0·0190	122·0000
2.	0·0625	3·1300	0·0140	34·0000
3.	0·0403	2·6500	0·0110	21·8000
4.	0·0543	2·7700	0·0120	17·0000
5.	0·0664	1·3700	0·0090	14·1000
4 & 5 combined	0·0610	2·2900	0·0010	16·5000

and the ratios, taking No. 1 as 1000, are :—

1.	1000	1000	1000	1000
2.	760	510	735	277
3.	492	431	575	178
4.	662	450	630	139
5.	810	224	473	115
4 & 5 combined	745	374	526	135

Here we see that there is a diminution of value pretty regular up to No. 3, when there is a rise in No. 4 and No. 5 in the temperature, a rise in No. 4 and a fall in No. 5 in the vapour and humidity, whilst the fall is progressive throughout in the carbonic acid.

In each case the result of the combination of 4 and 5 gives a number which takes its proper place after No. 3, except in the temperature.

The following Table gives the values according to the second method, viz. as reciprocal of the square of the probable error of the result :—

No.	Temperature.	Vapour.	Humidity.	Carbonic acid.
1.	5·2716	293·93000	1·2656	16378·2000
2.	4·1165	324·2300	0·5318	3750·4000
3.	3·7470	281·3300	1·0966	4148·1000
4.	3·7100	170·000	0·5439	2307·5000
5.	1·5839	34·2770	0·1986	674·3000
4 & 5 combined.	5·3171	195·8300	0·7708	2957·5100

and the ratios, taking No. 1 as 1000, are :—

1.	1000	1000	1000	1000
2.	781	1103*	420	229
3.	711	957	867	253
4.	704	578	432	141
5.	302	117	157	41
4 & 5 combined.	1008*	667	609	181

Here we see much the same order preserved, except that in two cases marked * (*Nos.* 4 *and* 5, *temperature, and No.* 2, *vapour*) the amounts exceed No. 1. It is also observable that in the vapour, humidity, and carbonic acid No. 3 is superior to No. 2. In every case the combined 4 and 5 series is superior to the two singly, being nearly their sum.

In all the Tables it may be observed that the humidity is somewhat irregular in relation to the amount of vapour. This may be understood from the fact that it is a complex quantity, depending partly on the amount of vapour, and partly on the temperature.

If we now seek to get a general expression of the relative values of all the observations in each *order*, we may take the product of their values by the different methods.

Table showing the products of the Values of each Order, calculated from the Reciprocals of the Squares of Mean Errors.

No. of Order.	Product.	Ratio.
1.	1·1720	1000
2.	0·0931	794
3.	0·0256	218
4.	0·0307	262
5.	0·0115	98
4 & 5.	0·0230	196

Table showing the same from Probable Error of Result.

No. of Order.	Product.	Ratio.
1.	32139057	1000·00
2.	2661995	83·00
3.	4794655	149·00
4.	791570	25·00
5.	7254	0·23
4 & 5.	2373680	74·00

Here we see a greater irregularity, No. 3 showing a superiority over No. 2, due probably to the greater number of individual observations in the former case.

Taking the mean of the ratios by the two methods, we have :—

No.			No.		
1	=	1000	4	=	144
2	=	439	5	=	49
3	=	184	4 & 5	=	135

But the discrepancy in the ratios of the values from the probable error, where No. 3 exceeds No. 2, is due to the irregularity in the humidity column; and as this is not an independent quantity, but dependent on the temperature and vapour, we may legitimately omit it. We shall then have the products as follows :—

Values from Mean Error.

No.	Value.	Ratio.
1.	61·40	1000
2.	6·65	108
3.	2·33	38
4.	2·56	41
5.	1·28	21
4 & 5.	2·30	38

Values from Probable Error of Result.

No.	Value.	Ratio.
1.	25382435	1000·00
2.	5005632	197·00
3.	4372692	172·00
4.	1455360	57·00
5.	36526	1·44
4 & 5.	3079500	121·00

It will be seen that in the calculation from mean error there is a rise at No. 4 in both instances, *i. e.* with and without the humidity. There is a fall at No. 5, whilst the combined series 4 and 5 gives a result which follows naturally after No. 3. We may now reject Nos. 4 and 5 as sepa-

rate orders, and consider them in combination, when we shall have the following relative values :—

No.		From Mean Error.		From probable Error of Result.
1.	......	1000	......	1000
2.	......	108	......	197
3.	......	38	......	172
4 & 5.	......	38	......	121

and the mean of the two values will be :—

No. 1........	1000	No. 3........	108
No. 2........	153	Nos. 4 & 5....	80

We have now a series of ratios which follow a regularly descending scale, very much in the order we might have expected *à priori*, seeing that the sense of smell is naturally less acute as the organic matter increases in amount. But it is of less consequence to determine the position of the higher orders in the scale, except as a measure of the general value of the observations throughout the inquiry, the really important point being the very great superiority of the first order, particularly as regards the carbonic acid. This is an additional argument for its adoption as the limit of admissible impurity in good ventilation.

The amount of fresh air necessary to keep the impurity down to the particular limit would be according to the following formula,

$$d=\frac{e}{\rho},$$

where d is the delivery of fresh air in cubic feet per head per hour, e the amount of carbonic acid expired per hour by one inmate, and ρ the limit of respiratory impurity taken as carbonic acid per cubic foot. If we take e to be 0·6 of a cubic foot in a state of complete repose, such as during sleep, we are rather under Pettenkofer's estimate, but considerably above Angus Smith's. The following Table gives the amounts necessary for the three estimates :—

No. of order.	Limit of respiratory impurity per cubic foot.	Cubic feet of air per head per hour calculated from		
		Angus Smith's estimate, e=0·450	Proposed estimate as adopted by Dr. Parkes, e=0·600.	Pettenkofer's estimate, e=0·705.
1.	0·0001831	2460	3280	3850
2.	0·0003894	1155	1540	1810
3.	0·0006322	710	950	1115
4 & 5.	0·0008533	530	700	825

I think that the general opinion is that Angus Smith's results give too low an estimate, and that 0·600 is really the lowest that can be with safety admitted.

The existing Army Regulations contemplate a delivery of 1200 cubic feet per head per hour in barracks; but practical inquiry has shown that this amount is generally fallen short of. The result is that the life of the soldier, at least during his sleeping-hours, is passed in a No. 3 air-space, or one in which the organic impurity is *decidedly disagreeable to the senses.* Previous to 1858 he did not even get this moderate amount of air; so that his life was spent in an air-space in which the organic matter was *offensive and oppressive to the senses.* If we adopt (as proposed already) 0·2000 per 1000 of CO_2 as the limit of impurity, then 3000 cubic feet per head per hour is the amount which must be delivered, on the supposition that e=0·600, or 3525 if e=0·705.

We may say, in conclusion, that the experimental data already quoted fairly justify the adoption of the following conditions:—

Conditions as the Standard of good Ventilation.

Temperature (dry bulb) 63° to 65° Fahrenheit.
,, (wet bulb) 58° to 61° ,,

N.B. The temperature should never be very much below 60°, but it may be found difficult to prevent its rising in hot weather. In any case the difference between the two thermometers ought not to be less than 4°, and ought not to exceed 5°.

Vapour ought not to exceed 4·7 grains per cubic foot at a temperature of 63 F., or 5 grains at a temperature of 65° F.

Humidity (per cent.) ought not to exceed 73 to 75.

Carbonic Acid. Respiratory impurity ought not to exceed 0·0002 per foot, or 0·2000 per 1000 volumes.

Taking the mean external air ratio at 0·4000 per 1000, this would give a mean internal air ratio of 0·6000 per 1000 volumes.

By considering separately the conditions found in barracks and in hospitals, or among healthy and among sick men, a point of some interest and importance seems to be indicated—namely, that more air is required for the latter than for the former to keep the air-space pure to the senses. This is due either to the greater quantity of organic matter or to a difference in its quality and nature. The following results are found from the data in the Tables:—

	Barracks.	Hospitals.
Mean amount of carbonic acid per 1000 volumes as respiratory impurity found when the air was noted as "fresh" &c., the impurity not being appreciable to the senses	0·196	0·157
Number of analyses in each group	75	38

Assuming the average carbonic acid per head to be 0·6 of a cubic foot, these amounts indicate a supply of air as follows:—

	Barracks.	Hospitals.
Amount of air supplied per head per hour in cubic feet	3062	3822

Stated in round numbers, therefore, we may say that while a barrack-room may be kept sweet with 3000 cubic feet, it will take 4000 to keep a hospital ward containing ordinary cases in the same condition. Much more would, of course, be required during times of epidemic or the like.

There is less regularity in the higher orders; but if the whole of the observations, other than No. 1, are taken together, we find a similar indication:—

	Barracks.	Hospitals.
Mean amount of carbonic acid per 1000 volumes, as respiratory impurity, in all the observations, when the organic matter was appreciable by the senses..	0·601	0·580
Number of analyses in each group................	274	86

Calculating the amount of air supplied as above, we have:—

	Barracks.	Hospitals.
Amount of air supplied per head per hour in cubic feet	998	1034

A comparison may also be made by attaching a numerical value to each order, which we may do by making the mean carbonic acid of No. 1 unity, and finding its ratio to the others thus:—

No. of order.	Mean respiratory impurity as CO_2.	Ratio, No. 1 being unity.	Differences.
1.	0·1830	1·00	...
2.	0·3894	2·13	1·13
3.	0·6322	3·46	1·33
4 & 5.	0·8533	4·66	1·20

The progression is pretty regular, and the mean difference is 1·22, which differs but little from the individual terms.

[*To face* p. 201.]

TABLE (*Summary*) showing the Mea Orders,

with the]

Details given in the columns under each h	ose" the hich num ren- the hed.	Nos. 4 & 5 combined, Being the probable limit of possible differentiation by the senses.
Mean difference between external and internal air		+0·8533
Mean error of series of observations		0·2465
Limits of mean error		+1·0998
		+0·6068
Error of mean square of a single observation		0·2949
Limits of do.		+1·1482
		+0·5584
Probable error of a single observation		0·1899
Limits of do.		+1·0432
		+0·6634
Error of mean square of the result..........................		0·0273
Limits of do.		+0·8806
		+0·8260
Probable error of the result..................................		0·0184
Limits of do.		+0·8717
		+0·8349
Actual observed mean in the external air		0·3928
Range in the inhabited air-space, calculated from th mean, *plus* the difference between the limits of :—		
a. Mean error		1·4926
		0·9996
b. Error of mean square of a single observatio		1·5410
		0·9512
c. Probable error of a single observation		1·4360
		1·0562
d. Error of mean square of the result		1·2734
		1·2188
e. Probable error of the result		1·2645
		1·2277
Actual observed mean in the inhabited air-space		1·2461
Modulus calculated from :—		
a. Mean error		0·437
b. Error of mean square of a single observatio		0·417
Ratio of modulus (*a*) to modulus (*b*)		100 : 95
Value of the series calculated as the reciprocal of the squ		
a. Mean error		16·5000
b. Probable error of result		2957·5100
Number of observations		117
Mean temperature of air-space		65°·12
Mean vapour in grains in a cubic foot of air..............		5·108
Mean humidity per cent.		74·05

Adopting the above numbers as the respective numerical values of each order, we have for *barracks* :—

No. of order.		No. of observations.		Value of order.		Total.
2.	......	89	×	2·13	=	189·57
3.	......	88	×	3·46	=	304·48
4 & 5.	......	97	×	4·66	=	452·02
	Sums....	274				946·07

giving a mean of 3·45.

For *hospitals* we have :—

2.	......	20	×	2·13	=	42·60
3.	......	46	×	3·46	=	159·16
4 & 5.	......	20	×	4·66	=	93·20
	Sums....	86				294·96

giving a mean of 3·43.

Here we find the same numerical value (signifying *close*) applied to 0·580 in hospitals and 0·601 in barracks. There is thus, even in this comparatively limited number of observations, a confirmation of the opinion that more air is necessary to keep an air-space sweet in disease than in health. It is, however, right to point out that in the one case the occupation was continuous, and in the other chiefly at night only.

II. "On the Atmospheric Lines of the Solar Spectrum, illustrated by a Map drawn on the same Scale as that adopted by Kirchhoff." By J. B. N. Hennessey, F.R.A.S. Communicated by Prof. Stokes, Sec.R.S. Received, the map June 9, 1874, the text January 11, 1875.

(Abstract.)

The spectroscopic observations described in this paper were made with instruments belonging to the Royal Society, and in accordance with certain suggestions which had been made to the author by a committee appointed in consequence of a letter of his to Sir Edward Sabine, President, dated 13th February, 1866. In view of his residence at a considerable height above the sea-level, and of the exceedingly clear atmosphere prevailing at some periods of the year, it was suggested that the locality was peculiarly favourable for a determination of the lines of the solar spectrum due to atmospheric absorption, and that, for this purpose, the solar spectrum when the sun was high should be compared with

TABLE (*Summary*) showing the Mean Difference between the External and Internal Air, as regards Carbonic Acid, in each of the Orders, with the Mean Errors, Errors of Mean Square, Probable Errors, Limits, Value, &c.

Details given in the columns under each head.	Carbonic Acid per 1000 volumes.					
	No. 1. "Fresh or not," differing sensibly from the external air.	No. 2. "Rather close"—that is, the point at which the organic matter begins to be appreciated by the senses.	No. 3. "Close" — that is, the point at which the organic matter begins to be decidedly disagreeable to the senses.	No. 4. "Very close"—that is, the point at which the organic matter begins to be offensive and oppressive to the senses.	No. 5. "Extremely close"—that is, the point at which the maximum point of differentiation by the senses is reached.	Nos. 4 & 5 combined, Being the probable limit of possible differentiation by the senses.
Mean difference between external and internal air	+0·1830	+0·3894	+0·6322	+0·8432	+0·8817	+0·8533
Mean error of series of observations	0·090	0·1715	0·2140	0·2425	0·2659	0·2465
Limits of mean error	+0·2740 +0·0910	+0·5609 +0·2179	+0·8462 +0·4182	+1·0857 +0·6007	+1·1476 +0·6158	+1·0998 +0·6068
Error of mean square of a single observation	0·1231	0·2528	0·2665	0·2882	0·3127	0·2949
Limits of do.	+0·3061 +0·0599	+0·6422 +0·1366	+0·8987 +0·3657	+1·1314 +0·5550	+1·1944 +0·5690	+1·1482 +0·5584
Probable error of a single observation	0·0831	0·1705	0·1797	0·1944	0·2109	0·1899
Limits of do.	+0·2661 +0·0999	+0·5599 +0·2189	+0·8119 +0·4525	+1·0376 +0·6488	+1·0926 +0·6708	+1·0432 +0·6634
Error of mean square of the result	0·0116	0·0242	0·0230	0·0309	0·0571	0·0273
Limits of do.	+0·1946 +0·1714	+0·4136 +0·3652	+0·6552 +0·6092	+0·8741 +0·8123	+0·9388 +0·8246	+0·8806 +0·8260
Probable error of the result	0·0078	0·0163	0·0155	0·0208	0·0385	0·0184
Limits of do.	+0·1908 +0·1752	+0·4057 +0·3731	+0·6477 +0·6167	+0·8640 +0·8224	+0·9202 +0·8432	+0·8717 +0·8349
Actual observed mean in the external air	0·4168	0·4110	0·3705	0·3903	0·4001	0·3928
Range in the inhabited air-space, calculated from the observed external mean, *plus* the difference between the limits of :—						
a. Mean error	0·6908 0·5078	0·9710 0·6289	1·2167 0·7887	1·4760 0·9910	1·5477 1·0159	1·4926 0·9996
b. Error of mean square of a single observation	0·7229 0·4767	1·0532 0·5476	1·2692 0·7362	1·5217 0·9453	1·5945 0·9691	1·5410 0·9512
c. Probable error of a single observation	0·6829 0·5167	0·9709 0·6299	1·1824 0·8230	1·4279 1·0391	1·4927 1·0709	1·4360 1·0562
d. Error of mean square of the result	0·6114 0·5882	0·8246 0·7762	1·0257 0·9797	1·2644 1·2026	1·3389 1·2247	1·2734 1·2188
e. Probable error of the result	0·6076 0·5920	0·8167 0·7841	1·0182 0·9872	1·2543 1·2127	1·3203 1·2433	1·2645 1·2277
Actual observed mean in the inhabited air-space	0·5998	0·8004	1·0027	1·2335	1·2818	1·2461
Modulus calculated from :—						
a. Mean error	0·162	0·304	0·379	0·430	0·471	0·437
b. Error of mean square of a single observation	0·174	0·358	0·377	0·408	0·443	0·417
Ratio of modulus (*a*) to modulus (*b*)	100 : 108	100 : 118	100 : 99	100 : 95	100 : 94	100 : 95
Value of the series calculated as the reciprocal of the square of :—						
a. Mean error	122·0000	34·0000	21·8000	17·000	14·1000	16·5000
b. Probable error of result	16378·2000	3750·4000	4148·1000	2307·5000	674·3000	2957·5100
Number of observations	113	109	134	87	30	117
Mean temperature of air-space	62°·85	62°·85	64°·67	65°·15	65°·05	65°·12
Mean vapour in grains in a cubic foot of air	4·629	4·823	4·909	5·078	5·194	5·108
Mean humidity per cent.	73·03	74·25	71·55	73·60	75·31	74·05

the spectrum at sunset, and any additional lines which might appear in the latter case should be noted with reference to Kirchhoff's map.

Accordingly the author set to work with the spectroscope first supplied to him, and in the autumns of 1868 and 1869 mapped the differences in question from the extreme red to D. These results appeared in the 'Proceedings of the Royal Society' for June 16, 1870, and the map of the spectra, sun high and sun low, of the region in question forms plate 1 of the 19th volume.

The instrument first supplied to the author was found in practice to be of insufficient power to permit of ready identification of the lines seen in the spectrum of the sun when high with those represented in Kirchhoff's map; and a new spectroscope of greater power was supplied to him, which reached him at the end of the year 1871. Observations for a continuation of his map had, in the mean time, been taken with the old instrument in the autumns of 1870 and 1871, and the spectrum mapped from D to F, in continuation of the former map. But the new instrument proved so superior to the old, that the author determined to map the whole spectrum afresh from observations made with it, using the former maps merely as skeleton forms. The observations with the new instrument were carried on in the autumns of 1872 and 1873, and the map now presented is the result.

Observations were also made to ascertain whether any of the lines which came out when the sun is low, especially those which are also seen, but narrower and less conspicuous, when the sun is high, could be due, not to *specific* atmospheric absorption, but to the general weakening of the light, causing parts of the spectrum already weakened by *solar* absorption to appear dark when a *general* weakening of the light was superinduced, though they had appeared bright when the light was strong. For this purpose the spectrum of the sun when high, as seen in the usual way, was compared with the spectrum when the intensity was artificially reduced in various ways. The best comparison was obtained by taking advantage of a natural phenomenon. At Mussoorie, late in the autumn, a haze, visible at sunset, extends over the low country, and grows day by day in height, till it causes the sun virtually to set in haze while still 3° or more above the horizon, whereas in the clear season it is visible till it attains a depression of $1\frac{1}{2}°$. The result of the comparison was, that none of the additional lines were discovered to have any other origin than selective atmospheric absorption.

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"On the alleged Expansion in Volume of various Substances in passing by Refrigeration from the state of Liquid Fusion to that of Solidification." By ROBERT MALLET, F.R.S. &c. Received April 28, 1874*.

The fact that water expands in becoming ice, and that the latter thus floats upon the water, can scarcely have escaped the observation or inference of the acute intellects of a remote antiquity. Its conditions, when more carefully examined in modern times, pointed out the strange and, as it has been called, anomalous fact that water can be cooled 7° or 8° below its freezing-point without becoming solid, and that between its maximum density at about 39° Fahr. and its freezing-point at 32° Fahr., or within the narrow range of 7° Fahr., it expands in the large ratio of 915 : 1000.

Standing thus alone amongst observed phenomena in nature, it seems to have suggested to many experimenters the question whether other bodies when liquefied by heat might not also expand when becoming solid by refrigeration. I have not attempted to trace with minuteness the history of past inquiry upon this subject, many loose uncertain statements as to which have for at least a century continued to perplex scientific literature. Réaumur appears to have been the first who gave currency to the statement that cast iron, bismuth, and antimony all expand in consolidating. The like fact has been alleged or left to be inferred with respect to the following substances by the authorities named:—

Silver, Persoz.

Copper, Karsten.

Mercury and Gold, as inferred by Nasmyth and Carpenter.

* Read June 11, 1874. See 'Proceedings,' vol. xxii. p. 366.

Iron and Furnace-slags, by experiment of Heunter and Snelus, as quoted by Nasmyth and Carpenter.

But of this list the only body, in addition to water, that really appears proved to expand in consolidating is bismuth; and even this the author cannot affirm upon the basis of his own experiments, but accepts the fact, at least provisionally, as true upon the uncontradicted statements of many chemical authors, and upon the positive assurance which he is permitted to mention by Dr. John Tyndall that he is satisfied of its truth. With respect to all the others, it is the object of this communication to show that the evidence in support of the alleged fact of expansion by refrigeration is illusory and insufficient, and to offer with respect to cast iron, and also with respect to iron furnace-slags, experimental proof of the untruth of the statement.

Certain connected but only collateral facts, having regard to so-called anomalous changes of volume due to temperature, will not be referred to here—such, for example, as the anomalous expansion of Rose's fusible metal, which expands progressively, like other bodies, till it attains the temperature of 111°; it then contracts rapidly by added heat to 150°, when it is densest (Graham's 'Elements,' vol. i., and Gmelin's 'Handbook'), the circumstances being here probably due to the successive segregation in the mass of alloys differing from each other in constitution, dilatability, and fusing-points. Or, again, the facts observed with respect to the expansion or contraction in volume shown by certain salts when crystallizing from their solutions, the whole of the conditions as to which have not been as yet made quite clear. The statement that antimony expands in consolidating, as made by Réaumur, has been negatived by Marx. The like statement with respect to silver and copper appears to rest on no better foundation than the observation as stated by Persoz, "that pieces of solid silver float upon the melted metal, showing that silver expands in solidifying like water" (Gmelin's 'Handbook,' vol. vi.). As to gold, there appears no authority whatever for its expansion on consolidation. Mr. Nasmyth has included it in his catalogue merely on the vague inference that, like silver and copper, it "exhibits surface-converging currents in the melting-pot like those depicted by him for molten iron" ('The Moon,' Nasmyth and Carpenter, p. 24), which, as we shall see further on, affords no grounds for conclusion on the matter. Réaumur's statement with respect to cast iron appears to have rested upon nothing more than the fact that he had observed certain pieces of cold cast iron to float upon cast iron while in fusion. Until lately this subject generally attracted but little attention, for it had very few, and these mere technical, applications; and to the higher physicist they presented but little interest, because the loosely stated facts, even if accredited, did not in the slightest degree tend to elucidate or explain the remarkable and perhaps still isolated facts as to water and ice. Accordingly, with little or no examination, the statements given for facts by the older authorities have been

accepted and become current from book to book of authors up to the present day, as when Dr. T. Thomson ('System of Chemistry,' vol. i. p. 375, 5th edit.) says of cast iron that "it contracts considerably when it comes into fusion," or that of Kerl ('Metallurgy of Iron,' Crookes and Röhrig's translation, vol. ii. p. 291), that cast "iron occupies a smaller space after cooling than when in the liquid state; it contracts in such a manner that, at the commencement of its solidification, it first expands so as to be able to fill up the smallest depressions and cavities of a mould, but after solidifying it contracts"—a loosely worded statement, which in various forms may be found in a great number of authors upon metallurgy and technology. So likewise the statement often repeated, that the value of antimony in type-metal consists in its causing the latter to expand upon consolidation and so perfectly fill the matrix, is presented, so far as the author's reading goes, without the slightest experimental proof of its truth, and appears to rest simply upon Réaumur's statement with respect to antimony itself, which, as already mentioned, has been controverted by Marx. This subject, however, has now assumed greater importance, since it has recently been made by Messrs. Nasmyth and Carpenter the foundation upon which they rest their theory of lunar volcanic action, as presented to us by the surface of our satellite; and the object of the present communication is to show that, as regards the two most pertinent of the substances adduced by these authors, viz. cast iron and iron furnace-slag, the facts entirely fail in support of their theory.

First, then, as to cast iron. It is not a fact that all cast iron in the solid state will float upon all cast iron in liquid fusion, though such might be inferred from the broad and loose statements of authors. Even in the limited form in which the statement is made by Nasmyth and Carpenter—viz. "that when a mass of solid cast iron is dropped into a pot of molten iron of identical quality the solid is found to float persistently upon the molten metal, so persistently that when it is intentionally thrust to the bottom of the pot it rises again the moment the submerging agency is withdrawn" ('The Moon,' p. 21)—is not quite exact.

It is a fact that certain pieces of cast iron in the solid and cold state will float on certain descriptions of cast iron in liquid fusion; but whether the solid pieces shall float or not float in any given case is dependent at least upon the following conditions, and probably upon others not yet ascertained:—

1st. Upon the relative specific gravities of the solid and of the fused cast iron both referred to the temperature of the atmosphere. Under the commercial name of cast iron is comprehended a wide range of compounds of iron with other substances, which compounds differ greatly in their physical as well as their chemical qualities, and have a range of specific gravity of from nearly 7·7700 for the whitest, most rigid, and dense, down to little more than 6·300 for those which are darkest, softest, and most porous. The total dilatation at the fusing-point of the denser

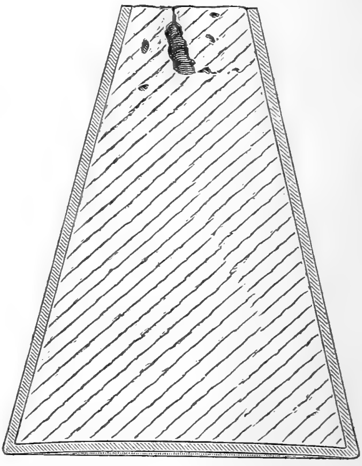
cast irons is known to be somewhat greater than that of the less dense; but as the increase in volume may not be sufficient to equalize the specific gravity of a very dense iron when in fusion with that of a very light iron when cold, so it is obvious that a piece of cold cast iron might be so selected in reference to its specific gravity, as referred to that of another sort of cast iron in fusion, that the former should either sink or swim upon the latter by mere buoyancy, were that free to act alone.

2nd. Assuming the cold and the molten cast iron originally identical in qualities, whether a piece of the former shall float or not float upon the latter depends not only upon buoyancy as above, but also upon the form of the piece of cold metal—that is to say, on the relation, all other things being the same, that subsists between its volume and its surface.

3rd. The force, whatever be its nature, which keeps the piece of cold cast iron floating is of sufficient energy to overcome a considerable want of buoyancy in the cold iron under certain conditions, so that it may float upon molten cast iron whose specific gravity, as such, is much less than that of the colder iron which floats upon it. Messrs. Nasmyth and Carpenter assume, without any sufficient proof, that solid cast iron floats on liquid iron of the same quality in virtue of buoyancy alone, and proceed to state that "the inevitable inference from this is that in the case of cast iron the solid is specifically lighter than the molten, and therefore that, in passing from the molten to the solid condition, this substance undergoes expansion in bulk" ('The Moon,' pp. 20, 21).

I proceed to prove that this view is altogether contrary to fact. The determination of the specific gravity of cast iron in its molten condition is a problem of considerable difficulty, and can only be solved by indirect means; we cannot ascertain its specific gravity by any of the methods ordinarily employed, nor can any areometric method be used, as any hydrometer or solid of known specific gravity at common temperatures, when dipped into liquid cast iron, changes its volume as well as gets incrusted with adherent cast iron or its oxides, &c. By an indirect method, and by operating upon a sufficiently large scale to eliminate certain sources of error, the specific gravity of molten cast iron may, however, be approximately ascertained with considerable accuracy. The method adopted by the author was as follows:—A conical vessel, of the form shown in figure 1, was formed of wrought-iron plate by welding up only,

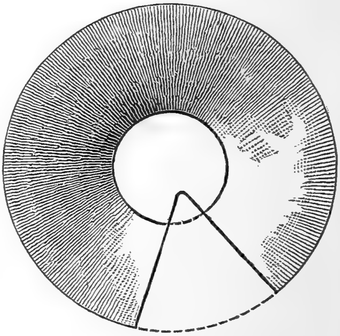
Fig. 1.



the walls of the vessel being about $\frac{1}{2}$ in. in thickness. It was perfectly smooth in the inside, and the plane of the lip of the open neck was carefully made parallel to the plane of the base. This vessel weighed, when empty, 184·75 lbs. avoirdupois. The orifice of the neck being levelled as the vessel stood upon the platform of the weighing-apparatus, it was filled up to the exact level of the neck with water at a temperature of 60°·5 Fahr., and again weighed. Deducting the weight of the empty vessel, the weight of its contents of water was found to be 94·15 lbs. avoirdupois. From the known volume and weight of the imperial gallon of distilled water, the capacity of the vessel was therefore at 60° Fahr.=2605·5 cubic inches. As a check upon the results, both as to weight and capacity, the water was measured into the vessel from accurate glass standards of volume. The water employed was that from the well at Messrs. Maudslay, Sons, and Field's Engine Works, Lambeth, where these experiments were conducted, and to whose liberality the author owes the means of having performed them. The specific gravity of this well-water did not very materially exceed that of distilled water, being about 1·0004; but if we apply the necessary correction, the weight of the contents of the iron vessel of distilled water at 60° Fahr. is 94·112 lbs. avoirdupois. The vessel being emptied, carefully dried and warmed, and stood upon a hard rammed bed of dry sand with its neck perfectly level as before, was now filled perfectly level to the brim with molten cast iron. As the temperature of the vessel itself rapidly rose by contact with the large mass of molten iron within it, and by its dilatation had its capacity enlarged, so the top surface of the liquid cast iron within it rapidly sank, fresh additions of molten iron being constantly made to maintain its top surface level with the brim. This was continued until the whole of the exterior of the vessel was seen to have arrived at a clear yellow heat, beyond which no increase to its temperature took place. At about twenty minutes after the molten iron was first poured into the vessel, this point was reached, the feeding in of additional iron being discontinued a few minutes previously. The whole being left to cool for three days, the vessel full of the now cold and solid cast iron was again weighed; on deducting, as before, the weight of the empty vessel, the weight of the cast iron which filled it was found to be 645·75 lbs., which, with certain corrections to be yet noticed, was the weight of cast iron which, when in the molten state, was equal to the capacity of the conical iron vessel in its expanded state due to its exalted temperature. We have now to determine what was the capacity of the vessel in this expanded state. The temperature at which cast iron melts may be admitted as about 2400° Fahr.; but as iron tapped from the cupola is always above its melting-point, we may admit that it was poured into the vessel at 2600° or 2700° Fahr., the surplus heat in the cast iron, whose mass was about four times that of the wrought-iron vessel which contained it, being given off in the first instance to heat the latter. The temperature at which

wrought iron presents to the eye a clear yellow visible in daylight may, in accordance with the views of most physicists, be taken as between the fusing-points of silver and of gold, or at 2000° Fahr. The mean coefficient of linear dilatation for 1° Fahr. of wrought iron has been determined between the limits of zero and 212° by Laplace, Smeaton, Troughton, and Dulong, the average of the four being 0·00000699 for 1° Fahr.; and this is certainly below the truth for the whole range of temperature up to fusion, as the rate of expansion of all fusible bodies appears to increase with the temperature. Rinmann has determined the linear dilatation of a bar of wrought iron, when raised from 60° Fahr. to a white or welding heat, to be $\frac{7}{560}$ of its length, or ·0125; and taking the total range of temperature here at 2400°, we have a mean coefficient of linear dilatation $=0{\cdot}0000052$ for 1° Fahr. This is a still smaller coefficient than the preceding; the author has, however, preferred to adopt it in order to avoid any pretence to exaggerate in his own favour the results arrived at. Applying, then, Rinmann's coefficient to the dimensions of the cone at 60° Fahr., and to its temperature (2000° Fahr.) when at the maximum, we are enabled to deduce the true capacity of the cone when expanded to the utmost and filled with molten iron, viz. =2691·77 cubic inches. The iron conical vessel was now cut off by a circular cut at the base and another up and down the side of the cone, and separated from the conical mass of iron that had filled it; the interior surface of the iron vessel was found in several places about the lower part of the cone in perfect contact with that of the cast iron which had filled it; but in other portions very slightly distant from it, as judged by the sound of a hammer upon the sides of the vessel before it was cut off. The cast iron was not adherent to the vessel anywhere. The cast-iron cone being thus laid bare, had a V-shaped piece cut out of it (in the "slotting"-machine), as shown in fig. 2, by two planes, each passing through the axis and meeting at an angle of about 60°. The conical mass proved perfectly sound and free from cavities or blow-holes anywhere, except very near the summit or neck, where there was found to be a hollow or cavity accidentally left during the feeding (as above described). By measurement the volume of this cavity was found to be = 5·5 cubic inches; assuming this cavity filled with iron of the same quality as the cone, the weight of the latter would be increased by 1·43 lb., making thus the corrected total weight of the solid cone of cast iron = 647·18 lbs. From the wedge-shaped piece cut out from the cone at half its altitude, and about halfway between the axis and circumference of the sector, a piece was cut out, the specific gravity of which,

Fig. 2.



taken by the usual methods, proved to be 7·170, which may be taken as the mean specific gravity at 57° Fahr. of the whole of the cast iron that filled the cone. Reverting now to the conical vessel which contained at 60° Fahr. 94·112 lbs. of distilled water, its capacity being 2605·5 cubic inches: this capacity was enlarged by expansion when filled with molten iron to 2691·777 cubic inches, so that the conical vessel when cold, if it had had the same capacity as when filled with liquid iron, would have contained 97·224 lbs. of distilled water. We have now all the elements necessary for calculating the specific gravity of the cast iron which filled the cone in its molten state, because we have the actual weights of equal volumes of distilled water and of molten iron. The final results, then, are, that whereas the cast iron which filled the cone had when cold (57° Fahr.) a specific gravity, as above given, =7·170, the same cast iron in its molten state, as poured into the cone, had a specific gravity of only 6·650—in this case thus proving that the density of cast iron in its liquid state is not greater but, on the contrary, very much less than that of the same cast iron at the temperature of the atmosphere. The quality of cast iron employed in this experiment was the fine, bright, close-grained metal usually employed by Messrs. Maudslay, Sons, and Field for their engine-castings, and consisted of

$\left.\begin{array}{l}\frac{1}{4}\text{ Gartsherrie,}\\ \frac{1}{4}\text{ Coltness,}\end{array}\right\}$ Scotch,

½ Best scrap*—all by weight.

It may be taken as a typical or medium example of all good grey cast irons. I have not been enabled to repeat this experiment with the white, rigid, and crystalline cast irons, such as are employed for projectiles and other purposes; but as it is a recognized fact amongst iron-founders that these irons expand in the range of temperature between solidity and liquidity *much more* than do grey irons, so we may justifiably conclude that the decrease of specific gravity by fusion of these hard cast irons would be in even a greater ratio than that shown by the above experiment on grey iron; and generally the author feels himself justified in concluding that it is not true that *any* cast iron is denser in the fused than in the solid state. Cold cast iron, therefore, does not float upon liquid cast iron of the same quality by reason of its buoyancy, but in virtue of some force which tends to keep it upon the surface of the molten metal in opposition to a very considerable want of buoyancy or tendency to sink by greater density on the part of the solid iron, which is, by the preceding results, $\frac{1}{13\cdot 8}$ of its weight, whatever that may be, and is probably even greater than this in the case of hard white cast irons. The author's chief object has been thus far rather to prove that the cause assigned by the writers already mentioned is *not* the true cause of the floating of solid upon liquid cast iron of the same quality. What *is* the nature of the force which pro-

* Disused and broken-up castings.

duces this curious phenomenon and often in direct opposition to gravity, is a different and a much more delicate and difficult inquiry, which he must leave to physicists to fully investigate. The following experiments, however, may be placed on record as tending to afford some little dawn of light upon the subject.

The following experiments were made with pieces of iron cast from cast iron of the same quality as that which filled the experimental cone, placed upon or immersed in molten cast iron of like quality with themselves, so far as such can be secured by "tapping" at nearly the same time from the same cupola charged with the same materials.

Before proceeding to describe these, it will be necessary to deduce from the cone experiment a mean coefficient of total cubic dilatation for the whole range between 60° and 2400° Fahr. for the grey cast iron employed in these experiments. The total dilatation was, as we have seen, such as reduced the specific gravity of the cast iron when cold ($=7{\cdot}17$) to 6·65 when in fusion. The cubic dilatation was therefore in the inverse ratio of these numbers, or as 1000 : 1078; and dividing this increase in volume by 2340° Fahr., the total range of temperature, we obtain for the mean coefficient of cubic dilatation of this grey cast iron for 1° Fahr. $=0{\cdot}0000333$, or approximately for its mean coefficient of linear dilatation $\frac{0{\cdot}0000333}{3}=0{\cdot}0000111$. These coefficients are nearly double those obtained by Roy and by Lavoisier for a range of temperature of 180° Fahr., viz. between 32° and 212°, which is quite what we should expect, as the coefficient of dilatation in all bodies increases with the temperature.

We have seen from what precedes that two forces at least are concerned in the phenomenon of cold cast iron floating upon the same when liquid, viz.:—

A. *Buoyancy* or its opposite, dependent upon the relation between the actual specific gravity of the cold metal and that of the liquid metal upon which it is placed, and whose absolute power for any given difference of specific gravity depends upon the *volume* only of the floating mass.

B. A *repulsive force* of some kind tending to repel the surfaces in contact of the hot and cold metals. Whatever be the form of the floating solid, this repulsive force can only be effective in producing flotation upon such surfaces of the floating solid as are parallel to the surface of the liquid metal, or at least so circumstanced that repulsions upon one surface, or part of a surface, are not equilibrated and nullified by repulsions upon others in the opposite direction. Thus if a parallelopiped float with one of its surfaces parallel to that of the liquid metal, the repulsions upon its immersed vertical sides, taken two and two respectively, are in opposite directions, and therefore nullified, and the bottom or horizontal surface is alone effective in producing flotation. So also if a cylinder float with its axis horizontal, the ends are ineffective, as is also all that portion of the cylindric surface immersed which is above the level of the horizontal diameter of the cylinder.

These preliminary explanations will enable us better to interpret the following experiments.

Experiment 1. An irregular piece, believed to be of hard and dense cast iron, and also a ball of about $2\frac{1}{2}$ in. diameter, believed to be of close-grained grey iron: both sunk to the bottom when thrown into the ladle of liquid iron, and remained for some time at the bottom; both, however, reappeared upon the surface when they had acquired a temperature sufficient to have fused off portions of their respective masses.

[In every fresh-lined ladle of liquid cast iron there are circumferential ascending and central descending currents in the metal, produced by the gases evolved from the lining, as hereafter fully explained. It is no doubt chiefly to these ascending currents that the heated ball in Experiment 1 owed its ascent to the surface; for if the heating took place in perfectly motionless cast iron, there seems no reason why the place of the sunken ball should change up to the moment of complete fusion*.]

Experiment 2. Two parallelopipeds, each $2'' \times 2'' \times 6''$, were cast of close grey iron; one of these was placed cold upon the surface of a large ladle of liquid iron of like quality; the other was heated as hot as it would bear without distortion, viz. to nearly a bright yellow heat, in a forge-fire, and then placed upon the surface of the liquid metal. Both pieces floated, and, as nearly as could be judged, both to the same height above the liquid, namely 0·1808 in. The volume of the cold piece being 24 cubic inches, the ratio of the immersed to the emergent portions was as 9·6 to 1, the effective surface upon which the repulsive force could act in producing floatation being 12 sq. in. Assuming that the heated piece has been raised from 60° Fahr. (the temperature of the cold piece) to 2000° Fahr., and applying the mean coefficient of cubic dilatation as above given to this range of temperature, viz. $2000° - 60° = 1940°$ Fahr., we find that its volume was enlarged to 24·75 cubic inches, or $= \frac{1}{32}$ of the volume when cold; and taking the specific gravity of the cold piece to have been 7·17 (see *ante*), that of the hot piece would be reduced to 7·10; the effective repellent surface was slightly enlarged in the hot piece, and the immersed volume was to the emergent volume as 9·66 : 1. The buoyancy of the heated piece had been increased, or, more correctly, its *negative* buoyancy had been decreased, as compared with that of the cold piece, but yet it has sunk deeper into the liquid iron in proportion to their respective volumes. We may therefore be justified in concluding that the repellent force which kept both pieces afloat is diminished in energy in some proportion as the difference in temperature between the liquid metal and the piece floating upon it is diminished, and that where the liquid and the floating pieces are alike in quality of metal, both the negative buoyancy and the repellent force must both disappear at the instant that the floating piece itself becomes liquid by heat abstracted from the molten metal.

* All passages printed in brackets take date from 20th December, 1874.

Experiment 3. Two cylindric pieces of the same grey cast iron and of the same diameter (=2·375″) were gently placed with their axes horizontal upon the surface of the molten iron, the one being at 60° Fahr., the other at about 300° Fahr.; they both floated with a segment of the cylinder whose versed sine was 0·31 in. emergent. The volume of either cylinder was 22·15 cubic inches, and the emergent was to the immersed volume as 1 : 8·4. The effective repellent surface in each case (or cylindric surface below the horizontal diameter) was 18·65 sq. in.; but if we suppose, as in fact we have done, that the repellent force, whatever be its nature, acts everywhere perpendicularly to surfaces of contact of the solid and liquid, then the effective repellent surface here is only the difference between the immersed surfaces of the cylinder below and above the horizontal diameter, or 9·3 sq. in. From this we may perhaps conclude that the repellent force is mainly dependent upon the extreme upper parts of the range of temperature between the liquid and the cold body, inasmuch as an augmentation in temperature of the latter of 300°, or about $\frac{1}{8}$ of the entire range between solidity and fusion of the cast iron, produces no very sensible alteration in the tendency to float of the pieces.

Experiment 4. Three circular disks of the same grey cast iron, each of 6″ diam. by 0·375″ in thickness, were provided each with a slender iron wire eye, cast into the centre of one surface, so that by a hooked wire they could be gently laid upon the surface of the liquid iron of their own quality. The lower surface and edge of one disk were left as it came clean from the sand, those of another were rusted by wetting with solution of sal-ammoniac, and those of the third were ground smooth and polished by the grindstone. When the three disks were in succession laid upon the surface of the molten iron, they all floated alike as nearly as could be judged, each sinking to one half the thickness of the disk, so that the immersed was to the emergent volume in the ratio of equality. We may conclude from this that the condition of the metallic surface of the solid cast iron has no material influence upon its flotation.

Experiment 5. Two circular disks, provided with eyes as in experiment 4, were prepared, the one being 6 in. in diam. by 0·375 in thickness, and the other 3 in. in diam. by 1·5 in. in thickness. The respective volumes of these two disks are the same, but the circular flat surfaces respectively are as 4 to 1. The surfaces of the two disks being as they came from the sand-mould, they were placed gently upon the surface of the molten iron: both floated with the same portion in altitude emergent. The larger and thin disk had, as stated in experiment 4, its emergent and immersed volumes in the ratio of equality [or the emergent was to the whole volume as 1 : 2]. In the smaller and thicker disk, the emergent volume was to the immersed volume as 1 to 7. [Or the emergent volume was to total volume as to 1 : 8; but 2 : 8 : : 1 : 4, or the emergent volumes are to the total volumes in each case respectively proportionate to the lower or repellent surfaces of the disk.]

Now the effective repellent surfaces are here those of the lower circles of the respective disks, and these surfaces are to each other in the ratio of 1 (the larger) to $\frac{1}{4}$. Whatever be the nature, therefore, of the repellent force, it seems to be proportionate to some function of the effective surface as already defined, and not to the immersed volume of the solid cast iron which floats upon a liquid less dense than itself.

In all these experiments the mass of the molten cast iron was large in proportion to the pieces placed upon it, and the surface was kept by careful skimming almost perfectly free from scoriæ or oxide. A good deal of difficulty exists in observing the phenomena in such experiments as these, owing to the glare and heat of the molten metal. Whatever light these five experiments may throw upon the nature of the force which produces flotation, the subject must as yet be viewed as very incomplete. There are some facts of which no complete explanation can be offered without further experimental study; such as, for example, that a piece of cold cast iron which floats on liquid iron of its own quality if forcibly thrust to the bottom and rapidly and at once released, rises again rapidly to the surface with all the *appearance* of a *buoyant* body, which it certainly cannot be.

From what precedes, however, we may summarize as follows:—

If F be the force which keep the solid iron floating, B the buoyancy $\pm$ of the solid piece, and R the repellent force, then, in the case of a piece floating upon molten iron of its own quality, B is always negative, and $F=R-B$, the value of R for any given case depending upon the *effective surface* of the solid, and that of B upon its volume, both being modified by the initial difference in temperature between the solid and liquid metals. In the case of the solid being placed on liquid cast iron differing in quality from it, B may be either positive or negative, and R still dependent upon the conditions already stated. Hence in any such case we may have

$$F=R-B \text{ or } =R+B.$$

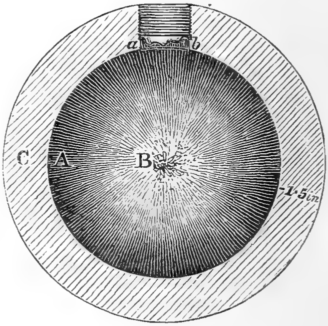
These conditions kept in view may clear up many phenomena at first apparently anomalous.

[However feeble may be the ascending currents, above referred to, upon the floating disks in experiment 5, their effect must be viewed as proportionate to the lower surfaces, and therefore proportionate to the repellent force, and as possibly adding, though slightly, to its effect.]

The following experiments were made at the Royal Arsenal, Woolwich, with a view to ascertain whether any sensible expansive force could be recognized as due to the enlargement in volume by consolidation of a spherical mass of cast iron:—Two spherical bomb-shells, each of about 10″ in diameter and 1″·5 in thickness, whose external orthogonal dia-

meters had been carefully taken when at atmospheric temperature (about 53° Fahr.), were both heated in an oven-furnace. One of these having been thus heated, but not to a *very* bright red, was permitted gradually to cool again, and its final dimension when cold noted. The other shell was withdrawn from the oven when at a bright red heat, and immediately filled to a little above the inner orifice of the fuse-hole (viz. to the line *a b*, fig. 3) with molten cast iron, the quality of this being the very dense mottled grey iron smelted at Elswick Works from the Riddesdale ores, and used in the arsenal for casting projectiles. The fuse-hole was closed by a screw-plug, which, however, did not reach within an inch of the surface of the molten metal, and the whole surrounded by a sheet-iron screen to keep off currents of air, was allowed to cool gradually, the dimensions being taken of the sphere as it cooled and contracted at intervals of half an hour until it had become cold. The enveloping shell was then cut through by the lathe in a great circle at right angles to the axis passing through the fuse-hole. One of the halves of the shell being detached, the interior surfaces of both hemispheres were found in perfect contact with that of the ball of iron they had contained, but no elastic tension seemed to exist in the shell. The ball of iron was drilled into and split by steel taper plugs, and sections of it exposed passing through the diameter in a line with the axis of the fuse-hole. There was no large cavity or "draw" anywhere in the interior, but there were two very small irregular cavities very near the fuse-hole; and the central portion of the mass embraced by an imaginary sphere of about 3″ in diameter, as shown in fig. 3, proved to be "spongy" and granular, as compared with the very dense and close-grained iron that constituted the remainder of the ball.

Fig. 3.

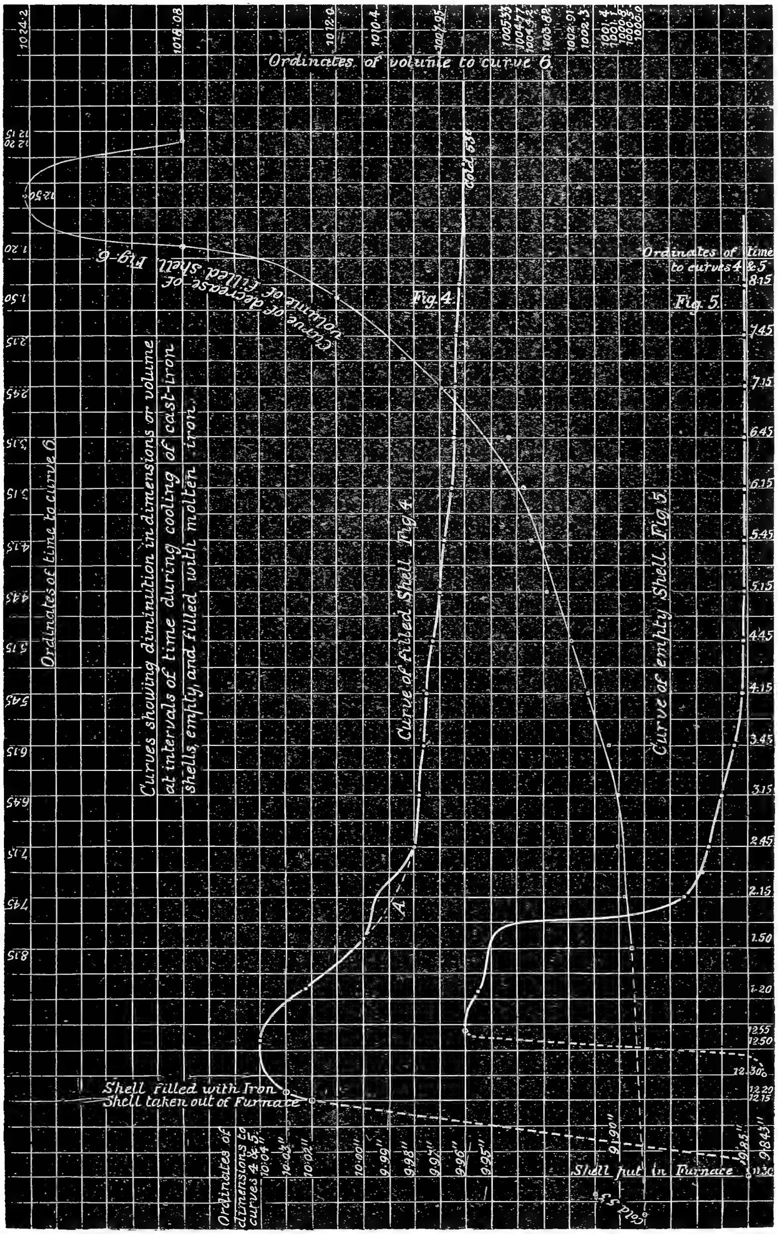


The following Table shows the course of contraction in dimensions of the filled shell and also of the empty shell in their progress of cooling, and the change in dimensions and in volume of the filled shell are shown by the curves (figures 4, 5, 6, p. 222):—

Time.		Diameter, filled shell.	Diameter, empty shell.
	Cold	9·850	9·843
11.30	Put in oven-furnace (shell to be filled)		
12.30	„ „ „ (empty shell)		
12.15	Withdrawn from furnace	10·020	
12.55	„ „ „	. .	9·960
	After filling with iron, diameter was	10·030 10·040	
12.50	„ „ „ „ „	10·040	
1.20	„ „ „ „ „	10·020	9·955
1.50	„ „ „ „ „	10·000	9·950
2.15	„ „ „ „ „	9·995	9·875
2.45	„ „ „ „ „	9·980	9·865
3.15	„ „ „ „ „	9·978	9·860
3.45	„ „ „ „ „	9·976	9·855
4.15	„ „ „ „ „	9·975	9·854
4.45	„ „ „ „ „	9·973	9·862
5.15	„ „ „ „ „	9·970	9·852
5.45	„ „ „ „ „	9·968	9·851
6.15	„ „ „ „ „	9·965	9·851
6.45	„ „ „ „ „	9·964	9·851
7.15	„ „ „ „ „	9·964	9·851
7.45	„ „ „ „ „	9·963	9·851
8.15	„ „ „ „ „	9·962	9·851
	When cold	9·960	9·851

The object of heating and cooling the empty shell was to ascertain what amount, if any, of permanent enlargement it might suffer, it being a well-known fact that all solids of revolution of cast iron, and generally of all metals of sufficient rigidity, become permanently enlarged by being heated red-hot and permitted to cool. This arises from the fact that the outer *couches* of the solid (a sphere for example) are the first heated and expanded, and have to draw off more or less from the less-heated mass within. Tangential thrusts and radial tensions are thus produced in the material of the outer *couches* which disappear, or even become reversed, as the progress of heating reaches the interior of the mass; but in the subsequent cooling the entire train of forces is reversed, the exterior *couches* lose heat by dissipation first, and have to accommodate by tangential tensions their dimensions to the still hotter interior, the final result being that when the whole has cooled the dimensions are greater than before the solid was heated. A 32-lb. spherical shot, which is rather more than 6 inches in diameter, can be thus permanently increased $\frac{1}{50}$ of an inch in diameter by a single heating. It is obvious that the increase will be much less in a spherical shell than in a solid sphere, and the less as

Figs. 4, 5, 6.


Ordinates of volume to curve 6
Ordinates of time to curve 6
Curves showing diminution in dimensions or volume at intervals of time during cooling of cast-iron shells, empty, and filled with molten iron.
Curve of decrease of volume of filled shell Fig. 6.
Fig. 4
Fig. 5
Ordinates of time to curves 4 & 5
Curve of filled Shell Fig 4
Curve of empty Shell Fig 5
Shell filled with Iron
Shell taken out of Furnace
Shell put in Furnace
Ordinates of dimensions to curves 4 & 5.
A

the shell is thinner*. On inspecting the Table and curve fig. 5, it will be seen that the empty shell had its diameter thus permanently enlarged by 0·008 of an inch; and had it been heated to as high a temperature as the filled shell, we may allowably conclude that this enlargement would have reached 0·01 of an inch. The filled shell has had its diameter increased by the decimal 0″·11; and if we deduct from this the amount of permanent enlargement due to heating only, equal to that of the empty shell, we have the decimal 0·11−0·01=0·10, which has to be otherwise accounted for. This shell was at a bright red heat visible in clear daylight when filled with the liquid iron, which occupied the spherical cavity and about 0·43 in height of that of the fuse-hole. The temperature of the shell visibly rose by the heat communicated from the liquid metal, and in 30 minutes after it was filled had attained its maximum, the surface being then at a bright yellow heat in daylight when the first measurement of enlarged diameter was made. The successive measurements were taken for orthogonal diameters in the direction normal to the fuse-hole by means of finely graduated steel beam calipers capable of being read to 0·002 of an inch or even less; the dimensions set down in the Table are the means of each pair of orthogonal diameters. The shell was thus heated at the commencement, and before consolidation of its liquid contents had taken place to any considerable extent, to within probably 200° or 300° Fahr. of the temperature of the cast iron within. The shell and its contents are therefore at the commencement very nearly in the same condition as though the whole were a sphere of molten iron without any more or less rigid envelope, if such could exist. Reverting to what has been said above as to the train of forces called into play in a cooling sphere, let us consider what has taken place here. As the heat is dissipated from the exterior of the molten mass, being transmitted through the shell, one *couche* after another of the molten metal in contact with the inner wall of the shell consolidates, the thickness constantly advancing towards the interior, where the metal is still liquid. If each of these *couches* in consolidating expanded in volume, such expansion must conspire, with the contraction constantly going on by the abasement of temperature, to produce compression in the central and as yet unsolidified portion of the mass. If, on the contrary, each *couche* as it solidifies contracts in volume (and, as is the fact, by a larger coefficient of contraction for equal small ranges of temperature before and after solidification), then the effect must be that, after the solidified crust has attained a certain thickness and sufficient rigidity, the further progress of contraction of the central portions as *they* successively solidify must be met by their tending to draw off from the solidified shell, or, in other words, by a drawing-off from each other of the particles of that central portion of

* For a more complete analysis of the complex strains brought into play by expansion and contraction in the heating and cooling of metallic solids of revolution, the author may refer to his paper "On the Coefficients T_e and T_r in large masses of Forged Iron," published in the Minutes of Proceedings Inst. C. E. London, vol. xviii. p. 299.

the sphere which last solidifies. Now the latter is exactly what has happened: a portion of the exterior and first solidified crust, reaching about an inch and half inwards from the interior of the shell, was found to have a specific gravity of 7·150 at 57° Fahr., while a portion taken close to the centre of the sphere had a specific gravity of only 7·037; and this specific gravity would have been still lower (or, in other words, the central part of the sphere would have been still more "spongy") had it not been fed by drawing downwards a portion of the liquid iron which partially filled the fuse-hole, the portion so drawn down being estimated by the volume of the cavities left at 0·400 of a cubic inch; so that but for this the specific gravity of the central spongy sphere taken at 3″ diameter would have been reduced to 6·776.

If we reduce this central spongy mass of 3″ diameter and of the last-mentioned specific gravity to a density as great as that found for the exterior crust, namely 7·150, the sphere of 3″ diameter would be reduced to one of 2″·138; and it is easy to see that in that case the external diameter of the whole sphere of metal and of the containing shell would have been less in a corresponding proportion, and that thus the final dimensions of the shell would have returned to what they were at the commencement, less the permanent enlargement, as measured by that of the empty shell. If there existed, on the other hand, any sensible expansion in volume of the metal in consolidating, not only would a central "spongy" portion be impossible and the central be the densest part of the whole sphere, but an enlargement of the entire mass and of the covering shell stretched by it must have occurred, so large as to be wholly unmistakable.

[The importance of the facts elicited from this experiment cannot be too forcibly laid before the reader. Had the sphere of molten iron, losing heat from its exterior, expanded in volume as *couche* after *couche* it solidified from the exterior, the solidification constantly advancing inwards, then the central portions of the sphere when ultimately solidified *must* be found to be the *densest* portions of the whole mass; the opposite of which was found to be the fact, the central portions of the experimental sphere being, as stated, the *least dense* portions of the whole mass. This alone seems conclusively to negative the supposition of any expansion in volume in cast iron in consolidating. On examining the curve fig. 4 in connexion with the Table, it will be remarked that between the hours 1.50 and 2.45 there is an irregularity in the progress of contraction which shows itself by a hump upon the curve which might be assumed to indicate a less rate of contraction within this epoch; and it might be further assumed that this apparent reduction arose from the conjoint action of general contraction and partial expansion operating together within some part of the mass; but this view, which the writer believes would be entirely incorrect, appears sufficiently negatived by the following considerations:—

1. Between the hours 1.50 and 2.45 but one caliper measurement was made, namely at 2.15, and upon this one measurement both the existence and the amount of this anomalous part of the curve depend. An error in this single caliper measurement amounting to 0·006 of an inch was sufficient to have produced it; and as the limit of reading of the beam calipers was to a limit of 0·002 or possibly 0·001 of an inch, a mistake in the measurement at 2.15, or a misreading of only the decimal ·004 or ·005 at most, is sufficient to account for the anomaly.

2. The hump on the curve does not necessarily indicate expansion, and from the early time of its occurrence, viz. only 1 hour 25 minutes from the commencement of cooling, it seems highly improbable that it could arise from partial expansion then commencing, while as yet a very large proportion of the entire mass must have been still liquid.

3. If this anomalous part of the curve were really due to expansion, it must have much more extensively affected the lower prolongation of the curve, and have shown itself there in a form that would have unmistakably declared its origin.

4. On examining the curve fig. 5 a slight anomaly may be remarked in the rate of contraction of the empty shell, due no doubt to some slight error in the third measurement, or that at 1.50 P.M. In this instance it would be impossible to ascribe the anomaly to expansion of any sort.

The dotted line A (fig. 4) may therefore be viewed as completing the curve of contraction.

The curve fig. 6, representing the volume of the filled shell at successive epochs of cooling, is deduced from the Table (p. 221), assuming the successive volumes to be proportionate to the cubes of the diametric measurements, the curve being a mean drawn through the several points of observation.]

The supposition upon which Messrs. Nasmyth and Carpenter's theory rests may be divided into two distinct propositions.

1st. That cast iron is of greater density in the molten than in the solid state.

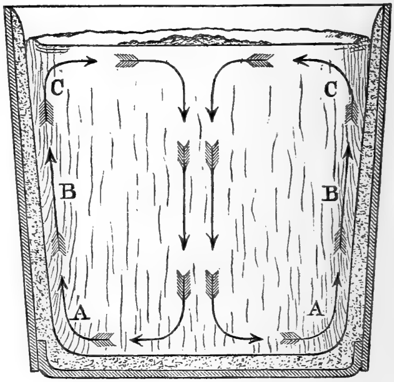
2nd. That cast iron in the act of consolidation expands in volume. These propositions are not identical, although the second is involved in the first. The first proposition has been already disposed of, and the last recorded experiments appear conclusively to disprove the second.

The phenomena described by Messrs. Nasmyth and Carpenter, and their explanation of the circulating currents observable in large and nearly cylindrical ladles of molten iron, *appear* at first sight so confirmatory of their views as to the greater density of cast iron in the molten than in the solid state, that it seems necessary here to present the true explanation of the facts, which, so far as they are here relevant, may be best given briefly in the words of these authors:—

"When a ladle of molten iron is drawn from the furnace and allowed

to stand at rest, the thin coat of scoria or molten oxide which forms on the surface of the metal is seen, as fast as it forms at the circumference of the ladle, to be swept by active convergent currents towards the centre, where it accumulates in a patch. As the fluid metal parts with some of its heat and the ladle gets hot by absorbing it, this remarkable surface-disturbance becomes less energetic." This arises from "the expansion of that portion of the molten mass which is in contact with the comparatively cool sides of the ladle, which sides act as the chief agent in dispersing the heat of the melted metal; careful observation will show that the motion in question is the result of an upward current of the metal around the circumference of the ladle, as indicated by the arrows A, B, C in the accompanying sectional drawing of the ladle" [fig. 7; the figure in the margin is essentially the same as that of these authors]. "The upward current of the metal can be seen at the rim of the ladle, where it is deflected into the convergent horizontal direction, and where it presents an elevatory appearance, as shown in the figure. It is difficult to assign to this any cause but that of expansion and consequent reduction of specific gravity of the fluid metal in contact with the sides of the pot, as, according to the generally entertained idea, the surface-currents above referred to would be in the contrary direction to that which they invariably take, *i. e.* they would diverge from the centre instead of converging to it."

Fig. 7.



The facts, so far as they are above described, are generally correct, but the explanation given is not the true one. The currents observable for some time after a large ladle (say, holding 10 tons) is first filled with molten iron are not produced by difference of temperature in different parts of the mass, but in the following way:—Such a ladle is of wrought iron, about half an inch in thickness; and to preserve this tolerably cool, even for several hours, it is lined with a coating of earthy material daubed upon the interior in a tough and plastic state, from an inch to an inch and a half in thickness, and dried within it. The lining material consists of plastic clay, with a proportion of siliceous sand beaten up together with horsedung, chaff, plasterer's cow-hair, or other fibrous material, conferring toughness upon the mass when soft and porosity when dry. This material, after drying at a temperature averaging 500° to 700° Fahr., on being exposed to contact with the molten cast iron, exhales torrents of gas and vapour, which pass upwards through the

molten mass and determine the direction of its currents; and it will be obvious, on inspecting the figure, that these currents will be most powerful round the outer circumference of the mass, where each unit of its top surface has a larger proportion of lining in proximity to it than at the central parts of the mass, where downward currents are the necessary consequence of those produced upwards at the circumference. The organic matters mixed with the lining are carbonized, and give forth the elements of water as well as nitrogen. The clay, which is a hydrous silicate of various earthy bases, gives forth its water and some of the oxygen of the peroxide of iron which most clays contain. More or less carbonate of lime is almost always interspersed, and this gives forth carbonic acid and water. The gases thus streamed forth act mechanically by their ascent and also chemically upon molten iron, the water being decomposed, oxidizing portions of the iron and forming scoriæ, which is again more or less reduced by contact of the hydrogen and nitrogen when the latter is present. These rapid combinations and decompositions are no doubt the main cause of those singular vermicular startings referred to by Messrs. Nasmyth and Carpenter, which are familiar to every iron-founder, but which are entirely distinct from the ascending and descending currents due to the ascent of the evolved gases. That this is the true explanation is supported by the following facts:—1. After a large ladle has stood full of molten metal for some hours, and time has been given thus for the whole of the gaseous contents of the lining to be driven off, the ascending and descending currents cease to be perceptible, and if any currents at all can be discerned they are in the opposite directions. 2. If, after this, such a ladle be emptied of its contents, the lining remaining untouched and only coated with a thin shell of adherent cast iron [and oxides and silicates of iron], and the ladle being again filled with molten iron, no such currents as at first are produced in the molten mass, the lining having been previously exhausted of its gases and vapours. That the currents described by Messrs. Nasmyth and Carpenter are *not* due to dissipation of heat from the mass through the sides of the ladle is evident from the following considerations:—

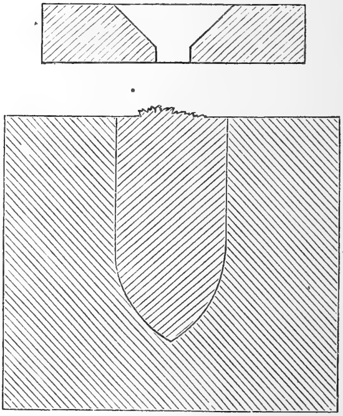
A 10-ton ladle, which is about $4\frac{1}{2}$ feet by 3 feet in depth, loses heat so slowly that after standing for six hours the molten metal is still fluid enough to make castings. Let us suppose it filled into the ladle at a temperature of 2800° to 2900° Fahr., and that after six hours it is still 200° above the temperature of solidification of cast iron, or at 2600°. The molten mass has thus lost 300° of heat in 360 minutes, or ·0138 of a degree per second. We may assume this at any instant as representing the difference in temperature between two vertical columns, one at the centre and the other at the circumference of the molten mass. The linear dilatation of cast iron for one degree of Fahrenheit being 0·0000111, as deduced from its total cubic dilatation between 60° Fahr. and the temperature of fusion at which it was poured into the cone, as given in this

paper, and assuming the depth of the colder of these columns, whether that be at the circumference or not, to be, as stated, 36″, that of the hotter column will be 36·0000005514, and the difference between these two measures the force which alone can produce circulating currents in the mass by differences of temperature due only to cooling. This is equally true whether it be the colder column that is dilated, as supposed by Messrs. Nasmyth and Carpenter, or the hotter one, as is the fact. And if we consider the viscidity of molten cast iron, it is perfectly obvious that the circulating currents referred to by Messrs. Nasmyth and Carpenter cannot be due to so insignificant a cause.

Want of attention, or careless interpretation of the many and somewhat complicated conditions thus seen to be involved in the cooling of a solid by dissipation of its heat from its exterior, has caused many serious misapprehensions on the part of experimenters as to the supposed expansion of metals in volume when consolidating. Thus, even in the case of bismuth, it has been supposed a conclusive proof of its expansion that a mass cooling in an open crucible exudes from its interior upon its top surface cauliflower-like excrescences; but although the author does not here deny or affirm any thing as to expansion being a fact in the case of bismuth, it is nevertheless obvious that such excrescences might arise merely from the grip of the crucible itself, or even of the exterior portions of the metal already solidified contracting upon and so squeezing out portions of the still liquid interior.

It is stated on good authority that a distinguished artillery officer, in former years at the head of the Laboratory, Woolwich Arsenal, satisfied himself of the reality of the expansion of cast iron in consolidating by the following experiment:—An elongated projectile was cast, with its axis vertical, in a very thick and massive mould of cast iron, the mould being cold or nearly so; the molten metal was introduced through a narrow aperture applied at the base of the projectile, as in fig. 8, the neck or "gate" being knocked off the instant the mould was filled. As the cooling rapidly proceeded, portions of the still fluid metal were forced out at the place where this neck was detached; and the conclusion was come to that the exterior being already solidified such excrescence could only arise from expansion of the contained liquid metal as it solidified in succession. What really did take place, and is the true explanation

Fig. 8.



of the facts, is, that when a very thick iron mould of this sort is suddenly heated by pouring molten iron into its interior, as the heat abstracted from the latter can only pass into the material of the mould at a rate determined by its conductivity, so the interior part rapidly becomes raised to a temperature enormously higher than the exterior portions, which for a time remain almost cold. The expanded interior walls of the mould push inwards as towards the points of least resistance, and so actually diminish the capacity of the mould for a time, the inner surfaces of which press upon the consolidating crust of metal within it, and so squeeze out in part its liquid contents, just as water might be squeezed from an india-rubber bottle*.

It seemed desirable to obtain some experimental results in reference to the objects of this communication with lead. It has never, so far as the author is aware, been even suggested that this metal expands in consolidating. Its coefficient of dilatation by heat is enormously greater than that of cast iron, being, according to the determination of Lavoisier and Laplace, between 32° and 212° Fahr. $=0{\cdot}0000474$ of its volume for one degree Fahr.; so that, taking its fusing-point at 617° (Rudberg), and assuming the coefficient constant for the entire range from 60° to 617° (which is much below the truth), its dilatation when in fusion would be $=0{\cdot}0264$ of its volume, and the specific gravity of lead at $60°=11{\cdot}36$; that of liquid lead must be *below* 11·07. Indeed this enormous amount of dilatation is impressed upon any observer who sees the rate at which the lead in casting a common bullet sinks into the neck of the mould, and the comparatively large cavity which always exists in the ball when cut in two. From its low temperature of fusion and the suddenness with which lead passes from the solid to the liquid state without any phase of intermediate viscidity, and only a brief one of crystalline brittleness, and the facility with which its surface can be kept free from dross or oxide, this metal presents a "crucial" example for experiment in reference to our subject.

The following experiments, by the kind permission of Messrs. Pontifex and Wood, London, were made at their works :—

1st. Upon the surface of a large pot of melted lead, the temperature

* The enormous disparity in temperature between the interior and the exterior *couches* of a very thick spherical or cylindrical iron mould is only partially shown by Biot's expression,

$$\log y=\log Y-\frac{x}{M}\sqrt{\frac{b}{a}},$$

for the distribution of temperature along a bar heated at one extremity; for in the case here before us, as in that of all forces radiating from a centre, the temperature at any given point, and quite independent of any question of conductivity, varies inversely as the square of its distance from the centre. If a unit in volume situated in any point of a radius lose as much heat as will lower its temperature 1°, that heat, when transmitted to double the distance along the radius, can only raise the temperature of a unit in volume there by 0·25 of 1° having been diffused amongst four units of volume. (See Biot, 'Traité de Physique,' and Mallet, 'Trans. R. I. A.,' 1856.)

of which was estimated at from 750° to 880° Fahr., the half of a large pig of newly smelted lead, being a semicylindrical bar of about $5'' \times 2\frac{1}{2}''$ and about 18″ long, was gently laid down horizontally; it immediately sank to the bottom and there remained. When about half its volume was melted away, the unfused portion was drawn up to the surface and let go, when it at once sunk to the bottom again.

2nd. A ball of such lead was cast, weighing $17\frac{1}{2}$ lbs., diameter about $4\frac{1}{2}''$; this was put into an empty hand-ladle, which was gently placed upon the surface of the pot of melted lead; the ladle was depressed sufficiently to fill with lead, and being left free was carried to the bottom of the pot with sufficient impetus to produce a sensible blow of the exterior of the ladle upon the bottom of the pot.

3rd. A flat circular disk of about 1·25 inch in thickness, being laid gently upon the surface, after a moment's hesitation slowly went to the bottom. Another disk of 6″ diameter, by rather less than an inch in thickness, remained a few seconds longer on the surface and then sunk to the bottom; both disks, while they floated, had their top surfaces but very slightly elevated above that of the liquid lead. One of the disks being gently lowered into the liquid lead vertically and edgeways, at once went to the bottom.

4th. Two disks, each 6″ diameter, the one 0·57 inch and the other 0·4 inch in thickness, being gently laid flat upon the surface of the molten lead, floated, and with an emergent portion sensibly greater than that of the disks in experiments 2 and 3, and remained floating until about 1·25 of the radius had been melted away all round, when they slowly sunk in the liquid, as was proved by the slow disappearance of the slender iron wire cast into the middle of the disk for the purpose of lowering. The thinner of these two disks floated rather longer than the thicker.

5th. A plate of sheet or laminated lead, clean from the rolling-mill, of about 0″·5 in thickness and about 10″ square, being gently placed flat on the surface of the liquid lead, floated, its top surface being nearly level with that of the liquid. After about ten seconds a piece was melted off from one of the edges, when the plate canted in the opposite direction and sunk.

6th. Plates of about 0″·18 thick floated much in the same manner as the preceding. The temperature of the solid lead employed was in all cases about 70° Fahr.

It follows from these experiments that, as in the case of cast iron, the solid does not float upon the liquid lead through buoyancy, that, on the contrary, the negative buoyancy is very marked, and that the repellent force, whatever be its nature, by which flotation is produced is dependent upon the effective surface as compared with the volume of the solid.

They present also a corroboration of the view that the repellent force upon the unit of effective surface is greater as the difference of temperature between the solid and liquid metal is so.

I proceed to some remarks upon the experiments referred to at the commencement of this paper, and quoted by Messrs. Nasmyth and Carpenter, as to the floating of pieces of solidified iron furnace-slag upon the same slag when in the liquid state. It is a fact that blast-furnace slags cooled below the point at which they become rigid do very generally float upon the same slag in its molten state. It is equally true that the basic silicates which constitute the chief part of terrestrial volcanic lavas float upon the surface of these when molten. But these admissions do not suffice in any degree to support the conclusion deduced by Messrs. Nasmyth and Carpenter, that basic silicates, whether as furnace-slags or lavas, are denser in the molten than in the solidified state, nor that these bodies in the act of solidification expand in volume or decrease in density in any manner, irrespective of the formation or enlargement of cavities or gas-bubbles within them. The experiments of the author upon the total contraction of iron furnace-slags for the entire range of temperature between that of the blast-furnace and the atmosphere, made at the Barrow Iron-Works, and fully described in the author's paper on "The Nature and Origin of Volcanic Heat and Energy," printed in Phil. Trans. for 1873, leave no doubt as to the following facts :—

1st. That the density of such slags at 53° Fahr. is to their density when molten and at the temperature of the blast-furnace as 1000 : 933, or, taken at the melting-point of slag, as 1000 to 983—molten slag being thus very much less dense than the same when solidified.

2ndly. That no expansion in volume whatever occurs in such slags at or near the instant of solidification.

The experiments of the author above referred to were made by filling cast-iron slightly conical moulds with the slag run direct from the blast-furnace, and permitted to consolidate and cool therein, by which perfectly solid slightly conical blocks were obtained. From the method employed, and the very large scale upon which these experiments were conducted, it is *impossible* that any expansion in volume at or near the point of consolidation, if even of a very minute amount, could have occurred and yet have escaped notice*. It is only necessary for the author here to point out that the floating of crusts of slag or lava is *not* due to the cause assigned by Messrs. Nasmyth and Carpenter; nor is it his intention to enter at any length into what are the causes of such floating when it occurs.

The following remarks, however, may be made :—It is impossible to obtain a moderate-sized fragment of solidified slag or lava free from air-bubbles, and from involved or superficial cavities, which tend to float the mass when thrown upon its own material in the melted state. Those who have attentively watched large volumes of slag issuing from the blast-furnace are aware that it comes forth carrying with it a large

* [For the proofs of which in detail the author begs to refer to his paper at length, Phil. Trans. 1873.]

volume of gaseous matter minutely diffused, which is pretty readily separated, and is characterized by a white vaporous cloud floating thinly over the issuing stream; if the slag be cooled rapidly, the gaseous or vaporizable bodies present become confined and render the mass vesicular, while if cooled more slowly, and with a free surface for the escape of these, the mass solidifies more solidly, often as solidly as a block of granite*.

Independently of the buoyancy that is produced by the vesicularity of rapidly cooled slags, it is highly probable that relatively cold and solid slag, whose buoyancy is negative, may yet float on molten slag, whose density is less than its own, in virtue of that same repellent force which, as we have seen, acts under like conditions in the case of metals.

With respect to acid silicates, or slags analogous to glass (which, however, are not referred to by Messrs. Nasmyth and Carpenter), the author again refers to the results given in his paper (Phil. Trans. 1873). These, and indeed the circumstances attending the production and destruction of the well-known "Rupert's drops," incontestably prove that these silicates also are *less* dense in the molten than in the solid state, and that they contract violently at or near the instant of consolidation.

The author has more than once heard the opinion expressed by those engaged about blast-furnaces, that their slags do expand in consolidating, based upon a misinterpretation of the following frequently occurring circumstance:—When the large parallelopipeds of slag (5 to 6 feet square by 2 to 3 feet thick) are stripped from the iron square frame which formed their edges, and are being removed upon the iron waggons on which they are cast, and still, as often happens, in a very hot state, or even with a still liquid or viscous interior, though rigid externally, it occasionally happens that such a block bursts asunder, and with a suddenness which is sufficient sometimes to scatter dangerously some of the liquid interior; or if the fracture be not so sudden, and the interior be in a viscous condition, the latter may continue for a considerable time to slowly exude in fantastic shapes from any aperture of escape left free to it (see fig. 9). These facts have been supposed to indicate that the interior of the mass expands in consolidating. It is scarcely necessary here, however, to enter

Fig. 9.

a. Tail of yet viscid slag extruding from interior.

* All this may be well observed in the millions of blocks of slag from the Cleveland blast-furnaces of which the vast embankments and breakwater at the estuary of the River Tees have been formed.

into any detail to prove that the phenomena are due to the contraction of the already solidified exterior upon the unyielding interior of the mass; the former becoming fractured by its own grip, and its material being highly elastic, often yields with apparently explosive violence like a suddenly broken spring (see fig. 9, which shows the appearance of one of these fractured and slowly exuding blocks).

[The following remarks may be made, in addition to those preceding, in contravention of the supposed expansion of slags or lavas in consolidating. It is well known that masses of mud when dried by the sun crack, the fissures penetrating nearly perpendicular from the surface and separating into more or less symmetrical prisms. Blocks of starch after desiccation present similar phenomena, which are also frequently seen exemplified by the uppermost beds of argillaceous limestone (or calp) of Ireland when first laid bare from its detrital covering. In all these cases there can be no doubt that the phenomena are due to the shrinkage of the mass in drying. But shrinkage or contraction by cooling and consolidation ought to present us with like results; and these we see actually manifest in the splitting-up of basalt into columnar prisms whose long axes are always found perpendicular to the surface by which the heat of the mass was dissipated. Such columnar separation is not confined to basalt; instances of it are abundant in lavas of every age, the surfaces of the prisms in these being sometimes straight, sometimes curved. Although much remains yet to be investigated before all the circumstances attending the splitting-up of masses of basalt or lava can be said to be fully understood, yet enough is already known and clearly explained to make it certain that it is due to *contraction* of these materials as they cool; and that this form of splitting-up is wholly incompatible with that of any fissuring that could arise from the refrigeration of a mass the volume of every part of which expanded in consolidating.]

As in what precedes the hypothesis upon which the lunar volcanic theory of Messrs. Nasmyth and Carpenter rests is proved to be without foundation, it seems needless to enlarge upon the incongruities and contradictions which the theory itself presents when fairly applied to such knowledge as we have of the volcanic features of the moon, or still more when applied, as it must be were it true, to those of our earth, [assuming the materials of our earth and satellite analogous in their physical and chemical properties—an assumption made by these authors throughout their work, though without any attempt to support it by proof].

In concluding this paper, the author has to express his thanks for the liberality and assistance afforded him by Messrs. Maudslay, Sons, and Field, who showed their just appreciation of the true value of scientific research by assisting in this inquiry of an abstract character, and without apparent technical applicability. He also has to thank Mr. E. Duncan, of the above firm, for his personal aid and cooperation. He also has to express his thanks for the valuable assistance so readily

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February 4, 1875.

JOSEPH DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Remarks on Professor Wyville Thomson's Preliminary Notes on the Nature of the Sea-bottom procured by the Soundings of H.M.S. 'Challenger.'" By William B. Carpenter, M.D., LL.D., F.R.S. Received Dec. 29, 1874.

The extreme interest of two of the questions started and partly discussed in Professor Wyville Thomson's communication will be deemed, I trust, a sufficient reason for my offering such contributions as my own experience furnishes towards their solution.

The first of these questions is, whether the *Globigerinæ*, by the accumulation of whose shells the *Globigerina*-ooze is being formed on the deep-sea bottom, live and multiply *on* that bottom, or pass their whole lives in the superjacent water (especially in its upper stratum), only subsiding to the bottom when dead.

Having previously held the former opinion, Prof. Wyville Thomson states that he has now been led to adopt the latter, by the results of Mr. Murray's explorations of the surface and sub-surface waters with the tow-net—which results concur with the previous observations of Müller, Haeckel, Major Owen, and others, in showing that *Globigerinæ*, in common with many other Foraminifera, have a pelagic habitat; while the close relation which they further indicate between the surface-fauna of any particular locality and the materials of the organic deposit at the bottom, appears to Prof. Wyville Thomson to warrant the conclusion that the latter is altogether derived from the former.

Now without in the least degree calling in question the correctness of these observations, I venture to submit, *first*, that they bear a different interpretation; and *second*, that this interpretation is required by other facts, of which no account seems to have been taken by Prof. Wyville Thomson and his coadjutor. In this, as in many other instances, I

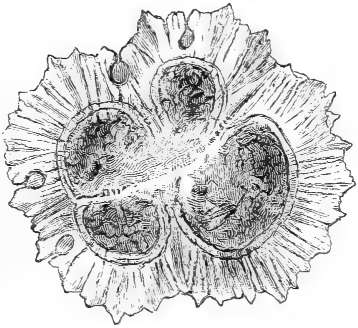
believe it will prove that the truth lies between two extreme views. That the *Globigerinæ live on the bottom only* is a position clearly no longer tenable; but that they live and multiply *in the upper waters only*, and only sink to the bottom after death, seems to me a position no more tenable than the preceding: and I shall now adduce the evidence which appears to me at present to justify the conclusion (I refrain from expressing myself more positively, because I consider the question still open to investigation), that whilst the *Globigerinæ* are pelagic in an earlier stage of their lives, frequenting the upper stratum of the ocean, they sink to the bottom *whilst still living*, in consequence of the increasing thickness of their calcareous shells, and not only continue to *live* on the sea-bed, but probably *multiply* there—perhaps there exclusively.

That there is no *à priori* improbability in their doing so, is proved by the abundant evidence in my possession of the existence of Foraminiferal life at abyssal depths. The collections made during the 'Porcupine' Expeditions of 1869 and 1870 yielded a large number of those *Arenaceous* types which construct their "tests" by the cementation of sand-grains only to be obtained on the bottom; and these were almost the only Foraminifera, except *Globigerinæ* and *Orbulinæ*, which came up in the 2435 fathoms dredging. Again, many Foraminifera, both arenaceous and shelly, were brought up from great depths, attached to shells, stones, &c., that *must* have lain at the bottom. Further, among the "vitreous" Foraminifera, the most common deep-sea types, except those of the Globigerine family, were *Cristellarians* with shells so thick and massive as to be (it may be safely affirmed) incapable of being floated by the animals which form them; while among the "porcellanous" Foraminifera, the *Biloculinæ* and *Triloculinæ* were equally distinguished by a massiveness of shell, which seemed to forbid the idea that they could have floated subsequently to that stage of their lives in which this massiveness had been acquired.

Of the existence of living *Globigerinæ* in great numbers in the stratum of water immediately above the bottom, at from 500 to 750 fathoms depth, I am able to speak with great positiveness. It several times happened, during the Third Cruise of the 'Porcupine' in 1869, that the water brought up by the water-bottle from immediately above the *Globigerina*-ooze was quite turbid; and this turbidity was found (by filtration) to depend, not upon the suspension of amorphous particles diffused through the water, but upon the presence of multitudes of young *Globigerinæ*, which were retained upon the filter, the water passing through it quite clear. The *thin shells* of these specimens, exhibiting very distinct pseudopodial orifices, contrasted strongly with the larger and thicker shells of the specimens brought up by the sounding-apparatus from the bottom immediately beneath, in which the shells are thick and those orifices obscure. It is obvious that if this extraordinary abundance of Globigerine life in the bottom-water was the result of subsidence from

the surface or sub-surface stratum, and was merely preparatory to the deposit of the shells on the sea-bed, there should have been a correspondence in size and condition between the floating shells and those lying on the bottom immediately beneath them; whereas no contrast could be more complete, the impression given by the superficial aspects they respectively presented having been fully confirmed by subsequent careful investigation.

Prof. Wyville Thomson and Mr. Murray, who notice this contrast, attribute it to the *death* of the shells which have subsided to the bottom—being apparently unaware that the observations of Dr. Wallich, with which my own are in entire accordance, leave no reasonable ground for doubt that it is a consequence of their continued *life*. For it is clearly shown, by making thin transparent sections of the thick-shelled *Globigerinæ* (an operation which needs a dexterity only to be acquired by long practice, and which is much facilitated by an ingenious device invented by Dr. Wallich *), that the change of external aspect is due to the remarkable *exogenous deposit* (a rudiment of the "intermediate skeleton" of higher Foraminifera) which is formed, after the full growth of the *Globigerina* has been attained, upon the outside of the proper chamber-wall—so completely masking its pseudopodial orifices, that Prof. Huxley at one time denied their existence. This deposit is not only many times thicker than the original chamber-wall, but it often contains flask-shaped cavities opening from the exterior, and containing sarcode prolonged into it from the sarcodic investment of the shell. Illustrations of this curious structure are given by Dr. Wallich in figs. 17 and 18 of Plate vi. of his 'North-Atlantic Sea-bed;' and I here subjoin a representation of it, kindly given me by Dr. Wallich twelve years ago, which further shows



Section of Shell of *Globigerina*, showing the distinction between the original proper wall of the chambers and the secondary exogenous deposit, with the flask-shaped cavities in the latter opening externally and containing sarcode like that which fills the chambers.

that the specimen from which it was taken had both its chambers and the flask-shaped cavities of the exogenous deposit filled with sarcode not distinguishable in any respect from that of the floating specimens. From these important observations (which had not been made public when the

* Ann. & Mag. of Natural History, 1861, viii. p. 58.

sheet of my 'Introduction to the Study of the Foraminifera' comprising the Globigerine family passed through the press, but which I have myself subsequently confirmed in every particular) it seems an almost inevitable inference that the subsidence of the *Globigerinæ* to the bottom is the consequence, not of their death, but of the increasing thickness and weight of their shells, produced by living action. As long as the number of segments continues to increase, the carbonate of lime separated by the sarcodic body from the circumambient water goes to form the walls of additional chambers; but when this chamber-formation ceases (which usually occurs when the shell consists of either 12 or 16 segments), it is applied to thicken the walls of the chambers already formed; and from the rapid subsidence of the *Globigerinæ* taken up from the sea-bottom when thrown into a jar of sea-water, it seems to me inconceivable that they can be floated by their animal inhabitants when once the exogenous deposit has attained any considerable thickness.

That the *Globigerinæ* which have subsided to the bottom continue to live there, is further indicated by the condition of the sarcodic contents of their shells. In any sample of *Globigerina*-ooze that I have seen brought up by the dredge or the sounding-apparatus, part of the shells (presumably those of the surface-layer) were filled with a sarcode body corresponding in condition with that of Foraminifera known to live on the sea-bed, and retaining the characteristic form of the organism after the removal of the shell by dilute acid. As Dr. Wallich pointed out ('North-Atlantic Sea-bed,' p. 139), the sarcode of these is viscid, and inclined to coalesce again when crushed; the shell has a vivid but light burnt-sienna colour, and sarcodic bosses, like retracted pseudopodia, are distinguishable upon its exterior. The only misgiving I ever had in regard to the living condition of the *Globigerinæ* presenting these characters, was caused by the absence of any pseudopodial extensions; and this source of doubt has been now removed by the statement of Prof. Wyville Thomson, that no pseudopodia have ever been observed by Mr. Murray to be put forth by the *Globigerinæ* captured in surface-waters.—In the same sample will be found shells distinguishable from the preceding by their dingy look and greyish colour, by the want of consistence and viscidity in their sarcode contents, and by the absence of any external sarcodic investment; these are presumably dead. Other shells, again, are entirely empty; and even when the surface-stratum is formed of perfect *Globigerinæ*, the character of the deposit soon changes as it is traced downwards. "The sediment," as was correctly stated by Prof. Wyville Thomson, "gradually becomes more compact; and a slight grey colour (due, probably, to the decomposing organic matter) becomes more pronounced, while perfect shells of *Globigerina* almost disappear, fragments become smaller, and calcareous mud, structureless and in a fine state of division, is in greatly preponderating proportion" ('Depths of the Sea,' p. 410). These facts seem to me to mark very strongly the

distinction between the *living* surface-layer and the *dead* sub-surface layer, and to show that there is nothing in the condition of the Deep Sea that is likely to prevent or even to retard the decomposition of the dead sarcode bodies of *Globigerinæ*. We know that oxygen is present in Oceanic water, even to its abyssal depths, in sufficient proportion for the maintenance of Animal life; and what suffices for this, must be adequate to promote the decomposition of organic matter. There is, moreover, a significant indication of the undecomposed condition of the sarcode bodies of the *Globigerinæ* of the surface-layer, in the fact that they serve as food to various higher animals which live on the same bottom. This was first pointed out by Dr. Wallich, who found that the contents of the stomachs of the *Ophiocomæ* brought up in his 1260 fathoms sounding, consisted of a number of fresh-looking *Globigerinæ* more or less broken up, minute yellow amorphous particles, and a few oil-globules ('North-Atlantic Sea-bed,' p. 145). And I have subsequently verified his statement in many other cases *.

It seems to me clear, from the foregoing facts, that the *onus probandi* rests on those who maintain that the *Globigerinæ* do *not* live on the bottom; and such proof is altogether wanting. The most cogent evidence in favour of that proposition would be furnished by the capture, floating in the upper waters, of the large thick-shelled specimens which are at present only known as having been brought up from the sea-bed. And the capture of such specimens would only prove that even in this condition the *Globigerinæ* can float; it would not show that they cannot also live on the bottom.

That the *Globigerinæ* not only *live*, but *propagate*, on the Sea-bottom, is indicated by the presence (as already stated) of enormous multitudes of very young specimens in the water immediately overlying it. And thus all we at present know of the life-history of this most important type seems to lead to the conclusion, that whilst in the earlier stages of their existence they are inhabitants of the upper waters, they sink to the bottom on reaching adult age, in consequence of the increasing thickness of their shells, that they propagate there (whether by gemmation or sexual generation is not known), and that the young, rising to the surface, repeat the same history.

I now proceed to show that the relation between the surface-fauna and the bottom-deposit is by no means as constant as Prof. Wyville Thomson and Mr. Murray affirm it to be.

It may be taken as proved that there is no want of Foraminiferal life in the Mediterranean. Prof. W. C. Williamson long ago pointed out

* Thus Man indirectly draws sustenance from the *Globigerinæ*; for the Cod which he fishes on the Faroe Banks chiefly live on the *Ophiocomæ* which swarm there, these again on the *Globigerinæ*, whilst the *Globigerinæ* seem to draw their sustenance from the organic matter universally diffused through sea-water, making it a *very* dilute broth!

that the "white mud" of the Levant is mainly a Foraminiferal deposit; I found a similar mud covering the bottom along the Tripoli coast; Mr. J. Gwyn Jeffreys has dredged Foraminifera in abundance in the Bay of Spezzia, Captain Spratt in the Ægean, Oscar Schmidt in the Adriatic, and I myself at various points in the Western basin along the northern coast of Africa. That Foraminifera, especially *Globigerinæ*, abound in its surface-water at Messina, is testified by Haeckel in the passage cited by Prof. Wyville Thomson; and when it is considered how large an influx of Atlantic water is constantly entering through the Strait of Gibraltar, and is being diffused throughout the Mediterranean basin, and how favourable is its temperature-condition, it can scarcely be doubted that if the doctrine now upheld by Prof. Wyville Thomson were correct, the deposit of *Globigerina*-shells over the whole bottom-area ought to be as abundant as it is in the Atlantic under corresponding latitudes. Yet I found the deeper bottoms, from 300 fathoms downwards, entirely destitute of Globigerine as of higher forms of Animal life; and this was not my own experience only, but was also that of Oscar Schmidt, who made a similar exploration of the Adriatic. In my first visit to the Mediterranean, in the 'Porcupine' (1870), many hundredweight of the fine mud brought up by the dredge from great depths in the Western basin were laboriously sifted, and the siftings carefully examined, without bringing to light more than a stray drift-shell here and there. And in my second visit, in the 'Shearwater' (1871), I examined all the samples of bottom brought up by the sounding-apparatus from great depths in the Eastern basin, with the same result—giving all the more care to this examination, because Capt. Nares (probably through not having kept separate in his mind the results of the deeper and of the shallower soundings which he had previously made in the Mediterranean) assured me that I *should* find minute shells imbedded in the mud.

I can see no other way of accounting for the absence of *Globigerina*-ooze from the bottom of the Mediterranean, save on its shallow borders, than by attributing it to the unfavourable nature of the influences affecting the *bottom-life* of this basin: that is to say, the gradual settling-down of the fine sedimentary deposit which forms the layer of inorganic mud everywhere spread over its deeper bottom, and the deficiency of oxygen and excess of carbonic acid which I have shown to prevail in its abyssal waters giving them the character of a stagnant pool—these influences acting either singly or in combination.

Another fact of which Prof. Wyville Thomson is fully cognizant, and to which he formerly attached considerable importance as indicative of the bottom-life of the *Globigerinæ*, is unnoticed in his recent communication: I refer to the singular limitation of the *Globigerina*-ooze to the "warm area" of the sea-bed between the North of Scotland and the Faroe Islands. It will be recollected by those who have read my 'Lightning' and 'Porcupine' Reports on the exploration of this region,

that whilst the whole upper stratum, from the surface to a depth of from 100 to 150 fathoms, has the temperature of the warm flow coming up from the S.W., and whilst this temperature falls so gradually in the "warm area" with increase of depth as to be still as high as 43° Fahr. at a depth of 600 fathoms, it falls so suddenly in the "cold area" between 150 and 300 fathoms, that the whole of its deeper stratum has a temperature below 32°, the bottom temperature descending in some parts to 29°·5. Now on this "cold area" I never found a single *Globigerina*, the bottom consisting of sand and gravel, and the Foraminifera brought up from it being almost exclusively those which form arenaceous tests. The "warm area," on the other hand, is covered with *Globigerina*-ooze to an unknown depth, its surface-stratum being composed of perfect shells filled with sarcode, whilst its deeper layers are amorphous. Near the junction of the two areas, but still within the thermal limit of the "warm," sand and *Globigerina*-ooze are mingled—this being peculiarly noticeable on the "*Holtenia*-ground," which yielded a large proportion of our most noteworthy captures in this locality. Now if the bottom-deposit is dependent on the life of the surface-stratum, why should there be this complete absence of *Globigerina*-ooze over the "cold area," the condition of the surface-stratum being everywhere the same? I was myself formerly disposed to attribute it to the depression of bottom-temperature; but as it has now been proved by the 'Challenger' observations in the Atlantic, that *Globigerina*-ooze prevails over areas whose bottom-temperature is but little above 32°, this explanation can no longer be accepted. And I can see no other way of accounting for it than by attributing it to the drift of the cold underflow, carying away the *Globigerinæ* that are subsiding through it towards the deep basin of the Atlantic, into which I believe that underflow to discharge itself. Prof. Wyville Thomson, however, denies any sensible movement to this underflow, continuing to speak of it as "banked up" by the Gulf-stream*, which

* See his 'Depths of the Sea,' p. 400. That there is a *lateral* pressure of the one flow against the other, just as there is a lateral pressure of the Labrador Current against the Gulf-stream on the North-American coast (producing the well-known "cold wall"), is sufficiently obvious from their relative distributions on the bottom of the channel. But it seems to me perfectly clear that the effect of this pressure is simply to *narrow* the glacial flow, and at the same time to increase its velocity. The most westerly point to which we traced it was near the edge of the Faroe Banks; and there (as Prof. Wyville Thomson himself pointed out to me at the time) the movement of the bottom-water was evidenced by the rounding into pebbles of what was elsewhere angular gravel. But it is even more conclusively shown by a comparison of the two serial soundings taken in the "cold area" (Nos. 52 and 64), which proves that the glacial stratum *flows up a slope* in the former position (just as the cold under-stratum does in the Florida Channel), which it could not do unless it were in movement. That we did not trace the outflow of this cold stream into the great basin of the Atlantic, was simply, as I believe, because we were prevented from ascertaining the bottom-temperature on the line which I expected that flow to take after surmounting the ridge.

here (according to him) has a depth of 700 fathoms; and this very striking example of want of conformity between the surface-fauna and the bottom-deposit consequently remains to be accounted for on his hypothesis.

The other of Prof. Wyville Thomson's principal conclusions, as to which I have rather a suggestion to offer than an objection to take, relates to the origin of the "red clay" which he found covering large areas in the Atlantic, and met with also between Kerguelen's Island and Melbourne. Into this red clay he describes the *Globigerina*-ooze as graduating, through the "grey ooze"; and he affirms this transition to be essentially dependent on the depth of the bottom. "Crossing," he says "from these shallower regions occupied by the ooze into deeper soundings, *we find universally* that the calcareous formation gradually passes into, and is replaced by, an extremely pure clay, which occupies, speaking generally, all depths below 2500 fathoms, and consists almost entirely of a silicate of the red oxide of iron and alumina. . . . . . . The mean maximum depth at which the *Globigerina*-ooze occurs, may be taken at about 2250 fathoms; the mean depth at which we find the transition grey ooze is 2400 fathoms; and the mean depth of the red-clay soundings is about 2700 fathoms. . . . . . . We were at length able," he continues, "*to predict the nature of the bottom from the depth of the soundings with absolute certainty* for the Atlantic and the Southern Sea." And from these data he considers it an indubitable inference "that the red clay is essentially the insoluble residue, the *ash*, as it were, of the calcareous organisms which form the *Globigerina*-ooze after the calcareous matter has been by some means removed." This inference he considers to have been confirmed by the analysis of several samples of *Globigerina*-ooze, "always with the result that a small proportion of a red sediment remains, which possesses all the characters of the red clay." Prof. Wyville Thomson further suggests that the removal of the calcareous matter may be due to the presence of an excess of carbonic acid in the bottom-waters, and to the derivation of this water in great part from circumpolar freshwater ice, so that, being comparatively free from carbonate of lime, its solvent power for that substance is greater than that of the superjacent waters of the ocean. He might have added probability to his hypothesis if he had cited the observations of Mr. Sorby as to the increase of solvent power for carbonate of lime possessed by water under greatly augmented pressure*.

Greatly struck with the ingenuity of this hypothesis, I turned to Prof. Wyville Thomson's tabular statement of the facts in detail; and must own to a great feeling of surprise at the want of conformity of these details with the assertions of universality and certainty of prediction which I have italicized in the above extracts. Thus in the deepest sounding in the whole Atlantic (that of 3875 fathoms, taken on the

* Proceedings of the Royal Society, vol. xii. p. 538.

voyage from St. Thomas to Bermuda), as well as in the next two soundings of 2960 and 2800 fathoms respectively (the average of the three being 3211 fathoms), the bottom was "grey ooze;" whilst in the next three soundings of 2850, 2700, and 2600 fathoms respectively (the average of the three being 2716 fathoms, or nearly 400 fathoms less than the preceding) the bottom was of "red clay." Between Bermuda and the Azores, again, there were six successive soundings between 2700 and 2875 fathoms, in which the bottom was "grey ooze."

It is clear, then, that no constant relation exists between depth and the nature of the bottom. If not only eight ordinary soundings whose average was almost exactly 2800 fathoms, but the extraordinarily deep sounding of 3875 fathoms, gave a bottom of "grey ooze," it surely cannot be "an ascertained fact that wherever the depth increases from about 2200 to 2600 fathoms, the modern chalk formation of the Atlantic and other oceans passes into a clay."

Now if this "red clay" had the character of an ordinary river-silt, it would be quite conformable to my Mediterranean experience to regard it (as Prof. Wyville Thomson himself was at first disposed to do) in the light of a derivative from the land, diffused through the ocean-water and slowly settling-down over particular areas, to which it might be determined by the prevalent direction of the bottom-flow, which would greatly depend in its turn upon the ridge-and-valley conformation of the sea-bed. And the presence of a small proportion of this material in the ordinary *Globigerina*-ooze, whilst, where it is deposited in quantity, there are neither entire *Globigerinæ* nor their disintegrated remains, would be perfectly consistent with the known destructive effect of the slow subsidence of a muddy sediment on many forms of animal life*.

But I agree with Prof. Wyville Thomson in thinking that the remarkable uniformity of this deposit, coupled with its peculiar composition, indicates a different derivation; and the suggestion I have to offer is based on its near relation in composition, notwithstanding its great difference in appearance, to *Glauconite*—the mineral of which the green sands that occur in various Geological formations are for the most part composed, and which is a silicate of peroxide of iron and alumina.

It is well known that Prof. Ehrenberg, in 1853†, drew attention to the fact that the grains of these green sands are for the most part, if not entirely, *internal casts* of Foraminifera—the sarcodic bodies of the animals having been replaced by glauconite, and the calcareous shells subsequently got rid of, either by abrasion or by some solvent which does not attack their contents. It was soon afterwards shown by Prof. Bailey (U. S.) that in certain localities a like replacement is going on at the present time, the chambers of recent Foraminifera being occasionally found to be

* See my 'Shearwater' Report in Proceed. Roy. Soc. 1872, vol. xx. p. 584.

† "Ueber den Grünsand und seine Erläuterung des organischen Lebens," in Abhandl. der königl. Akad. der Wissensch. zu Berlin, 1855, p. 85.

occupied by mineral deposit, which, when the shell has been dissolved away by dilute acid, presents a perfect internal cast of its cavities. By the application of this method to Mr. Beete Jukes's Australian dredgings, my coadjutors, Messrs. W. K. Parker and T. Rupert Jones, obtained a series of internal casts of most wonderful beauty and completeness, on which I have based my interpretation of the organic structure of *Eozoon Canadense*. Having myself examined in the same manner a portion of the Foraminiferal sand dredged by Capt. Spratt in the Ægean (kindly placed in my hands by Mr. J. Gwyn Jeffreys), I have found that it yielded a great variety of these beautiful models, not only of the bodies of Foraminifera, but also of the sarcodic network which interpenetrates the calcareous network of the shell and spines of *Echinida**.

Alike in Mr. Jukes's and in Capt. Spratt's dredgings, some of these casts are in *green* silicates and some in *ochreous*, corresponding precisely to the two kinds of fossil casts described by Prof. Ehrenberg. The difference I presume to depend upon the degree of oxidation of the iron; but as these casts are far too precious to be sacrificed for chemical analysis, I cannot speak with certainty on this point.

As it is only in certain limited areas of the sea-bottom that this replacement of the sarcodic bodies of *Foraminifera* by mineral deposit is met with, it has always seemed to me next to certain that there must be some peculiarity in the composition of the sea-water of those areas (produced, perhaps, by the outburst of submarine springs highly charged with ferruginous silicates) which gives to them a capability that does not exert itself elsewhere; and this now seems yet more probable from the circumstance that, notwithstanding the vast extent over which the 'Challenger' soundings and dredgings have been prosecuted, only two or three cases of the kind have been noted—those, namely, of the "greenish sands" brought up from 98 and 150 fathoms in the region of the Agulhas Current and in one or two other localities. It is a fact of peculiar interest, moreover, that the calcareous shells should have here disappeared, just as they have done in ordinary green sand; and this, too, although the depth was so small as altogether to forbid the idea that their disappearance is due to any solvent process brought about by the agencies to which Prof. Wyville Thomson attributes the removal of the calcareous deposit generated by Globigerine life.

Now in the residue left after the decalcification of Capt. Spratt's dredgings, I noticed a number of small particles of *red clay*, some of them presenting no definite shape, whilst others approximated sufficiently closely in form and size to the green and ochreous "internal casts" to induce me to surmise that these also had been originally deposited in the chambers of Foraminifera—their material being probably very nearly the same,

* Of these I hope to be able, ere long, to give a detailed account, in illustration of the similar models of the animal of *Eozoon* obtained by the decalcification of its serpentine lamellæ.

although its state of aggregation is different. And if this was their real origin, I should be disposed to extend the same view to the red clay of the 'Challenger' soundings; for a strong *à priori* improbability in the supposition that this is the "ash" of the shells themselves is created by the fact that we have no knowledge (so far as I am aware) of the presence of any such ash in calcareous organisms of similar grade. It is certainly not proved by the analyses of *Globigerina*-ooze quoted by Prof. Wyville Thomson; since this (supposing it to be free from any extraneous admixture) may have contained many shells partially or completely filled with such deposit. The only analysis that could prove it would be that either of shells of floating *Globigerinæ*, which may be presumed to be alive, or of those found in the surface-layer of the *Globigerina*-ooze, which (whether living or dead) have their chambers filled with sarcode.

I submit, then, that if the red clay is (as I am disposed to believe) a derivative of the *Globigerina*-ooze, its production is more probably due to a *post mortem* deposit in the chambers of the Foraminifera than to the appropriation of its material by the living animals in the formation of their shells. That deposit may have had the character, in the first instance, of either the green or the ochreous silicate of alumina and iron, which constitutes the material of the internal casts, and may have been subsequently changed in its character by a metamorphic action analogous to that which changes felspar into clay. That the presence of an excess of carbonic acid would have an important share in such a metamorphosis appears from the fact, long since brought into notice by Sir Charles Lyell*, of the disintegration of the granite in Auvergne and of the gneiss in the alluvial plains of the Po where subject to its influence. And the same agency (especially when operating under great pressure) would be fully competent to effect the removal of the calcareous shells, as was distinctly pointed out nearly thirty years ago by Prof. W. C. Williamson in his classical Memoir on the Microscopic Organisms of the Levant Mud†. This seems to me the most probable mode of accounting for their disappearance from a deep-sea deposit, where no mechanical cause can be invoked. But in shallower waters, where the same excess of carbonic acid does not exist, and the aid of pressure is wanting, but where a movement of water over the bottom is produced by tides and currents, I am disposed rather to attribute the disappearance of the shells to mechanical abrasion, having noticed, in Capt. Spratt's Ægean dredgings, that many of the shells were worn so thin that the coloured mineral deposit in their interior could be seen through them—which was, in fact, what first drew my attention to its presence. This is the explanation I should be disposed to give of the disappearance of the shells from the green sand brought up by the 'Challenger' in the course of the Agulhas Current; but whether it was mechanical abrasion or chemical solution that removed

* Principles of Geology, 11th ed., vol. i. p. 409.

† Memoirs of the Literary and Philosophical Society of Manchester, vol. viii. p. 98.

the Foraminiferal shells whose internal casts formed the Greensand deposit of the Cretaceous epoch, must remain for the present an open question*.

II. "Report to the Hydrographer of the Admiralty on the Cruise of H.M.S. 'Challenger' from July to November 1874." By Prof. Wyville Thomson, F.R.S., Director of the Civilian Scientific Staff on Board. Received January 4, 1875. (Published by permission of the Lords of the Admiralty.)

H.M.S. 'Challenger,' Hong Kong.

The 'Challenger' left Port Nicholson on the 7th of July, 1874, and proceeded under sail along the east coast of New Zealand. On the 8th we rounded and trawled in 1100 fathoms, lat. 40° 13′ S., long. 177° 43′ E., with a bottom-temperature of 2° C. and a bottom of soft greenish ooze. Many animals were brought up by this trawl resembling closely those which we had taken at a corresponding depth in other portions of the southern sea. On the 10th we again trawled and sounded in 700 fathoms about 40 miles to the east of East Cape.

We then continued our course northwards towards the Kermadec Islands, and on the 14th we took our usual series of observations midway between Macauley and Raoul Islands in the Kermadec group. At this station we trawled at a depth of 630 fathoms; and we were greatly struck with the general resemblance between the assemblage of animal forms brought up in the trawl and the results of a good haul in about the same depth off the coast of Portugal or North Africa. Among the more interesting objects were a very large and splendid specimen of a Hexactinellid sponge allied to *Poliopogon*, several other fine sponges referred to the same group, and three or four examples of two species of *Pentacrinus* new to science, resembling generally *P. asteria*, L., from the Antilles. We trawled on the following day in 600 fathoms, 45 miles to the north of Raoul Island, with nearly equal success. On the evening of Sunday the 19th we arrived at Tongatabu and called on the principal missionary, Mr. Baker, from whom we received every possible attention during our short stay. After spending two days in visiting different parts of the island, we left Tongatabu on the 22nd of July, and after taking a few hauls of the dredge in shallow water we proceeded towards Kandavu in the Fijis. On the 24th we stopped off Matuku Island and landed a party of surveyors and naturalists; and while they were taking

* It is due to Prof. W. C. Williamson to point out that, in the Memoir already referred to, he indicated the probability "that many of our European Greensands, and other siliceous strata, however barren of such structures they appear, may have once contained multitudes of calcareous microscopic organisms, some of which have been removed *after* the consolidation of the strata, leaving either hollow casts, or having had the cavities subsequently filled with silica."

observations and exploring on shore we trawled in 300 fathoms, and procured among other things a fine specimen of the Pearly Nautilus (*Nautilus pompilius*), which we kept living in a tub for some time in order to observe its movements and attitudes.

On Saturday, the 25th of July, we arrived at Kandavu, on the 28th we went to Levuka, and on the 3rd of August we returned to Kandavu, where we remained until the 10th.

At Fiji the civilian staff were occupied in examining the reefs and generally in observing the natural history of the islands; and in this we received all friendly assistance from H.M. Consul Mr. Layard and from Mr. Thurston, minister of King Cacobau. During our stay, a mixed party of naval and civilian officers went in the ship's barge to Mbaw and visited the king.

Between New Zealand and the Fiji group only two soundings were taken to a greater depth than 1000 fathoms. Of these, one, at a depth of 1100 fathoms off Cape Turnagain, New Zealand, gave a bottom of grey ooze and a bottom-temperature of 2° C.; and the second, at 2900 fathoms, lat. 25° 5′ S., long. 172° 56′ W., midway between the Kermadecs and the Friendly Islands, gave "red clay" and a temperature of 0°·5 C. Four serial temperature-soundings were taken; and the distribution of temperature was found to correspond, in its main features, with what we had previously met with in oceans communicating freely with the Antarctic sea.

The dredgings, which, with the exception of one near the New-Zealand coast, were all at depths varying from 300 to 600 fathoms, yielded a great number of very interesting forms; but, as I have already remarked, they tended to confirm our impression that, even at these comparatively moderate depths (at all depths, in fact, much greater than a hundred fathoms), while species differ in different localities, and different generic types are from time to time introduced, the general character of the fauna is everywhere very much the same.

On the 10th of August we left Kandavu and proceeded towards Api, one of the least known of the New Hebrides, where there is as yet no permanent missionary station. On the 12th we sounded and trawled in 1350 fathoms, with a bottom of reddish ooze; we sounded again on the 15th in 1450 fathoms, with red clay; and on the 18th, after passing through the channel between Makuru and Two-Hill Islands, we stopped off Api in 25 fathoms, close to the edge of the reef and opposite a landing-place.

In order to ensure, as far as we could, the good-will of the natives, Captain Nares had given a passage to eleven Api men, who had been employed for a three-years' term in Fiji, under the arrangement which exists there for the regulation of Polynesian labour. Two or three of us, with an armed party, took the returned labourers ashore; and as the natives (although they appeared somewhat mistrustful and were all armed

with clubs, spears, and bows with sheaves of poisoned arrows) were sufficiently friendly, nearly all the officers landed and spent a few hours rambling about the shore. It was not thought prudent to go far into the forest, which was very dense and luxuriant and came close down to the beach.

The natives were almost entirely naked, and certainly bore a very savage and forbidding aspect. One of them was manifestly greatly superior to the others, and appeared to exercise a considerable influence over them. He wore trowsers and a shirt and a felt hat, and could speak English fairly. He recognized me, at once, as having seen me at the sugar-plantation in Queensland, where he had been for the usual three-years' engagement, and showed me, with great pride, a note from his former employer, saying that the bearer was anxious to return to his service, and that he would willingly pay his passage-money and all expenses, in case of his being given a passage to Brisbane. I had been paying some attention to the South-Sea labour question, and had formed a very strong opinion of the value to the inhabitants of these islands of the opportunity given them, by this demand for labour, of testing their capacity to enter into and mix with the general current of working men, and thereby possibly avoid extermination; and I was greatly pleased to see the result in this instance.

From the island of Api we shaped our course to the north-westward towards Raine Island, in a breach of the great barrier reef not far from the entrance of Torres Strait. On the 19th of August we sounded, lat. 16° 47′ S., long. 165° 20′ E., at a depth of 2650 fathoms, with a bottom of "red clay" and a bottom-temperature of 1°·7 C. (35° F.). A serial temperature-sounding was taken to the depth of 1500 fathoms; and it was found that the minimum temperature (1°·7 C.) was reached at a depth of 1300 fathoms, and that consequently a stratum of water at that uniform temperature extended from that depth to the bottom.

Serial temperature-soundings were taken on the 21st, the 24th, the 25th, the 27th, and the 28th of August in 2325, 2450, 2440, 2275, and 1700 fathoms respectively; and in each case the minimum temperature of 1°·7 C. (or a temperature so near it as to leave the difference within the limit of instrumental or personal error of observation) extended in a uniform layer, averaging 7000 feet in thickness, from the depth of 1300 fathoms to the bottom.

It will be seen by reference to the chart that on our course from Api to Raine Island we traversed for a distance of 1400 miles a sea included within a broken barrier, consisting of the continent of Australia to the west, the Louisiade archipelago, the Solomon Islands, and a small part of New Guinea to the north, the New Hebrides to the east, and New Caledonia and the line of shoals and reefs which connect that island with Australia to the south. The obvious explanation of this peculiar distribution of temperatures within this area, which we have called, for

convenience of reference, the "Melanesian Sea," is that there is no free communication between this sea and the outer ocean to a greater depth than 1300 fathoms, the encircling barrier being complete up to that point.

The "Melanesian Sea" is in the belt of the S.E. trade-winds, and the general course of a drift-current which traverses its long axis, at an average rate of half a knot an hour, is to the westward; evaporation is, as it is usually throughout the course of the trade-winds, greatly in excess of precipitation, so that a large amount of the surface-water is removed. This must, of course, be replaced, and it is so by an indraught of ocean-water over the lowest part of the barrier, at the proper temperature for that depth. We had previously found a temperature of 1°·7 C. at a depth of 1300 fathoms on the 16th, the 19th, and the 21st of June between Australia and New Zealand, on the 17th of July in lat. 25° 5′ S., long. 172° 56′ W., and earlier on the 10th of March in lat. 47° 25′ S. The bottom within the Melanesian Sea may be described generally as "red clay," with a small but varying proportion of the shells of Foraminifera, sometimes whole, but more usually much broken up and decomposed. In one or two soundings the tube showed curiously interstratified deposits, differing markedly in colour and in composition. The trawl was sent down on the 25th of August to a depth of 2440 fathoms. The animals procured were few in number—some spicules of *Hyalonema*, a dead example of *Fungia symmetrica*, two living specimens of a species of *Umbellularia* (which appears to differ in some respects from the Atlantic form), and a very fine and perfect *Brisinga*, also living. The existence of animal life is therefore not impossible in the still bottom-water of such an enclosed sea; but, as we have already seen in the Mediterranean, the conditions do not appear to be favourable to its development. On the 29th of August we trawled in 1400 fathoms, about 75 miles to the east of Raine Island, with somewhat greater success. This might have been anticipated, as the depth was not much greater than that at which the free interchange of water was taking place, and diffusion and intermixture were no doubt much more rapid than at the bottom

On the 31st of August we visited Raine Island, which we found to correspond in every respect to Jukes's description in the "Voyage of the 'Fly.'" We observed and collected the species of birds which were breeding there. In the afternoon we dredged off the island in 155 fathoms, with small success, and proceeded towards Port Albany, Cape York, where we arrived on the 1st of September.

We left Somerset on the 8th, and proceeded across the Arafura Sea to the Arú Islands, reaching Dobbo. on the island of Wamma, on the 16th. We found no depth in the Arafura Sea greater than 50 fathoms, and the average depth was from 25 to 30 fathoms. The bottom was a greenish mud, due apparently to a great degree to the deposit from the great rivers of New Guinea and the rivers falling into the Gulf of Carpentaria.

Animal life was not abundant. Many of the animals seemed dwarfed, and the fauna had somewhat the character of that of a harbour or estuary. The specific gravity of the surface-water was unusually low, falling on the 23rd, off Dobbo Harbour, to 1·02505, the temperature reduced to 15°·5 C., distilled water at 4° C. =1.

After spending a few days shooting Paradise-birds and getting an idea of the natural history of the island of Wokaw, we left Dobbo on the 23rd and proceeded to Ké Doulan, the principal village in the Ké group. We then went on to the island of Banda, where we remained a couple of days, and thence to Amboina, which we reached on the 4th of October.

On the 26th of September, after leaving the Ké Islands, we sounded and trawled in 129 fathoms. The trawl brought up a wonderful assemblage of things, including, with a large number of Mollusca, Crustacea, and Echinoderms of more ordinary forms, several fine examples of undescribed hexactinellid sponges, and several very perfect specimens of two new species of *Pentacrinus*. Temperature-soundings were taken on the 28th of September and on the 3rd of October, at depths of 2800 and 1420 fathoms respectively; and on both occasions the minimum temperature (3° C.) was reached at a depth of 900 fathoms, indicating that the lowest part of a barrier inclosing the Banda Sea, bounded by Taliabo, Buru, and Ceram on the north, the Aru Islands on the east, Timor and the Salwatty Islands on the south, and Celebes and the shoals of the Flores Sea on the west, is 900 fathoms beneath the surface.

From Amboina we went to Ternate, and thence across the Molucca passage into the Celebes Sea, by the passage between Bejaren Island and the north-east point of Celebes. On the 13th, we trawled and took serial temperatures near Great Tawallie Island. The trawl brought up several specimens of a very elegant stalked halichondroid sponge new to science, and the thermometer gave temperatures sinking normally to a bottom-temperature of 2°·04 C. On the following day we sounded in 1200 fathoms, with again a normal bottom-temperature of 1°·9 C. It seems, therefore, that the Molucca passage communicates freely with the outer ocean; it does so at all events to the depth of 1200 fathoms, and most probably to the bottom, if it include greater depths.

In the Celebes Sea we had two deep soundings—on the 20th, to 2150 fathoms, and on the 22nd, to 2600 fathoms. On both occasions serial temperature-soundings were taken, and on both the minimum temperature of 3°·7 C. (38°·7 F.) was reached at 700 fathoms. A passage of this depth into the Celebes Sea is therefore indicated, very probably from the Molucca passage. This temperature corresponds almost exactly with that taken by Captain Chimmo in the same area. We trawled on the 20th; and although the number of specimens procured was not large, they were sufficient to give evidence of the presence of the usual deep-sea fauna.

We reached Zamboanga on the 23rd, and on the 26th we passed into the Sulu Sea and trawled at a depth of 102 fathoms. On the 27th we sounded to 2550 fathoms, and took a serial temperature-sounding. A minimum temperature of 10° C. was found at 400 fathoms; so that the Sulu Sea must be regarded as the fourth of this singular succession of basins, cut off by barriers of varying height from communication with the ocean. This observation in the main confirmed those of Captain Chimmo in the same locality. The minimum temperature reached was the same in both, but we appear to have found it at a somewhat higher level.

We arrived at Ilo Ilo on the 28th, and proceeded by the eastern passage to Manilla, which we reached on the 4th of November.

The collections have been packed and catalogued in the usual way, and will be sent home from Hong Kong. We have had an opportunity during this cruise of making a very large number of observations of great interest. I believe I may say that the departments under my charge are going on in a very satisfactory way.

February 11, 1875.

JOSEPH DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Structure and Development of *Myriothela*." By Prof. Allman, F.R.S. Received February 5, 1875.

(Abstract.)

The *endoderm* of the body is composed of numerous layers of large spherical cells composed of clear protoplasm, enclosing a nucleus with some brown granules and refringent corpuscles. Externally it is continued in an altered form into the tentacles, while internally it forms long thick villus-like processes which project into the cavity of the body. Towards the free ends of these processes there are abundantly developed among the large clearer cells, smaller, easily isolated spherical cells, filled with opaque brown granules. Where the endoderm passes into the tentacles it loses its large clear-celled condition, and consists of small round cells, so loaded with opaque granules that the axis of the tentacle appears nearly white under reflected light.

The free surface of the endoderm carries, at intervals, long, very slender, sluggishly vibrating cilia, and is overlaid by a thin layer of homogeneous protoplasm, which on the villus-like processes becomes especially distinct,

and which here develops minute mutable pseudopodia, which are being constantly projected and withdrawn. Indeed the vibratile cilia appear to be but a modification of these pseudopodial processes of protoplasm.

Interposed between the endoderm and the ectoderm is the *fibrillated layer*. It is extremely well developed, and consists of longitudinal muscular fibrillæ, closely adherent to the outer surface of a structureless hyaline membrane—the "Stützlamelle" of Reichert. The fibrillated layer, with its supporting membrane, is so strong as to remain entire in a section of the animal after the tissues on both sides of it have been broken down.

The *ectoderm* is composed of two zones, a superficial and a deep. The superficial zone consists mainly of two or three layers of small round cells containing yellowish granules. Among these cells the thread-cells may be seen, lying chiefly near the outer surface of the body. Two forms of thread-cells may be here distinguished—one ovate, with the invaginated tube occupying the axis; the other fusiform, with the invaginated tube oblique.

The deeper zone of the ectoderm consists of a very remarkable tissue, composed of peculiar membraneless cells, each of which is prolonged into a tail-like process, so that the cells assume a claviform shape. In most situations, where this tissue is developed, the processes from several such cells unite with one another, so as to form branching, somewhat botrylliform groups, whose common stalk can be followed into the fibrillated layer. The author is thus enabled so far to confirm the observations of Kleinenberg on cells of apparently the same significance in *Hydra*. In *Myriothela*, however, these cells do not, as in *Hydra*, reach the surface. With the exception, apparently, of their condition in the transitory arms of the *Actinula* or locomotive embryo, they form everywhere a deep zone interposed between the muscular layer and the superficial layer of the ectoderm. This zone is designated by the author as the zone of *claviform tissue*. Though it is in intimate association with the fibrillated layer, the author did not succeed in tracing a direct continuity of the individual fibrillæ with the processes of the cells, as described by Kleinenberg in *Hydra*.

The author adopts, as a probable hypothesis, the views of Kleinenberg respecting the caudate cells of *Hydra*, which he regards as representing a nervous system. While the deep layer of ectodermal cells in *Myriothela* would thus constitute a nervous layer, the superficial layer would represent an epidermis; and since recent researches justify us in regarding the ectoderm and endoderm of the Cœlenterata as respectively representing in a permanent condition the upper and lower leaf of the blastoderm in the development of the higher animals, we should thus find *Myriothela* offering no exception to the general law, which derives both epidermic and nervous tissues from the upper leaf of the blastoderm.

The structure of the tentacles is in the highest degree interesting. In

x 2

their narrow stalk-like portion, the condition of the endoderm departs widely from that of this tissue in the tentacles of other marine hydroids; for it presents no trace of the septate disposition so well marked in these. It is, on the contrary, composed of a layer of small cells loaded with opaque granules and surrounding a continuous wide axile cavity.

It is, however, in the terminal capitulum of the tentacle that the structure of these organs departs most widely from any thing that has as yet been recognized in the tentacles of other hydroids. Here a very peculiar tissue is developed between the muscular layer and the proper ectoderm, where it takes the place of the zone of claviform tissue. It forms a thick hemispherical cap over the muscular lamella and endoderm of the tentacle, and is composed of closely applied exceedingly slender prisms, with their inner ends resting on the muscular lamella, to which the prisms are perpendicular, the whole structure forcibly suggesting the rod-like tissue associated with special sense-apparatus in higher animals. It appears to be but a modification of the tissue which elsewhere forms the zone of claviform tissue.

Extending in a radiating direction from the convex surface of this rod-like tissue, towards the external surface ef the tentacle, may be seen numerous firm filaments, each of which, making its way among the cells of the ectoderm, terminates distally in a very delicate transparent oviform sac, which carries, near its distal end, a minute styliform process. Within this sac, and completely filling it, is an oviform capsule with firm transparent walls, and having immersed in its clear refringent contents a cylindrical cord wound upon itself in two or three coils. Under pressure, the contained cord may be sometimes forced out through the smaller or distal end of the capsule. Notwithstanding the obvious resemblance of these bodies to thread-cells, their significance is, withont doubt, something entirely different. Indeed their resemblance to the Pacinian bodies of Vertebrata is too strong to be overlooked. Their assemblage constitutes a zone parallel to the spherical surface of the capitulum, and lying at a slight distance within it. Though it is impossible to assign to them, with certainty, their exact function, we feel compelled to regard the whole system, including the bacillar tissue to which their stalks can be traced (and which is only a locally modified portion of the nervous zone, or zone of claviform tissue), as an apparatus of sense. It would almost seem to represent a form of sense-organ, in which sight and touch show themselves in one of their earliest phylogenetic stages, in which they have not yet become fully differentiated from one another. This is the only known instance of the existence in a hydriod trophosome of any thing which may with fair reason be regarded as a special apparatus of sense.

The male and female sporosacs are borne by the same trophosome.

The generative elements, whether male or female, originate in a special cavity (gonogenetic chamber), which is formed in the substance of the endoderm of the sporosac.

In the female, the primitive plasma becomes gradually differentiated into a multitude of cell-like bodies having all the characters of true ova with their germinal vesicle and spot. They are entirely destitute of enveloping membrane.

These bodies next begin to coalesce with one another into numerous roundish masses of protoplasm, which develop over their surface minute pseudopodial retractile processes.

The masses thus formed still further coalesce with one another; and there results a single spheroidal plasma mass, through which are dispersed numerous small spherical vesicles, mostly provided with a nucleus. These vesicles appear to be nothing more than the nucleolated nuclei of the coalesced ovum-like cells.

About the time of the completion of this last coalescence, the resulting plasma mass, enveloped in an external, very delicate, structureless membrane, is expelled, by the contraction of the sporosac, through an aperture formed by rupture in its summit.

Immediately after its expulsion, it is seized, in a manner which forcibly suggests the supposed action of the Fallopian tube on the mammalian ovum at the moment of its escape from the Graaffian follicle, by the sucker-like extremities of certain remarkable bodies, to which the author gave the name of *claspers*, which are developed among the blastostyles, and resemble long filiform and very contractile tentacles.

It is apparently now that fecundation is effected; for the plasma becomes again resolved into a multitude of roundish masses. This phenomenon may be regarded as representing the yelk-cleavage of an ordinary ovum. Reasons are assigned for believing that it is through the agency of the claspers that fecundation takes place; and the claspers are compared to the hectocotylus of Cephalopods, and to certain organs by which fecundation is effected among the Algæ.

The mulberry-like mass thus formed, surrounded by its structureless membrane, which has now acquired considerable thickness and forms a firm capsule, continues to be held in the grasp of the claspers during certain subsequent stages of its development. An endoderm and ectoderm with a true multicellular structure become differentiated, a central cavity is formed by excavation, and the germ becomes thus converted into a spheroidal non-ciliated *Planula*. This, after acquiring certain external appendages, ultimately escapes, by the rupture of the capsule, as a free actinuloid embryo.

The actinuloid, on its escape from its capsule, is provided not only with the long arms already noticed by Cocks and Alder, but with short scattered clavate tentacles. The short clavate tentacles become the permanent tentacles of the fully developed hydroid; the long arms, on the other hand, are purely embryonic and transitory.

The long embryonic arms originate in the spheroidal *Planula*. They are formed by a true invagination, and at first grow inwards into the

body-cavity of the *Planula*. It is only just before the escape of the actinuloid from its capsule that they evaginate themselves and become external.

After enjoying its free existence for one or two days, during which it moves about by the aid of its long arms, the embryo fixes itself by its proximal end, the long arms gradually disappear, the short permanent tentacles increase in number, and the essential form of the adult is soon acquired.

II. "Some Particulars of the Transit of Venus across the Sun, December 9, 1874, observed on the Himalaya Mountains, Mussoorie, at May-Villa Station, Lat. 30° 28′ N., Long. 78° 3′ E., Height above Sea 6765 feet."—Note No. I. By J. B. N. Hennessey, F.R.A.S. Communicated by Prof. Stokes, D.C.L., Sec. R.S. Received January 2, 1875.

May Villa, 9th December, 1874.

Naturally sharing in the great interest excited by the transit of Venus, which occurred this forenoon, I proposed that I should observe the event with the equatoreal of the Royal Society, which Capt. J. Herschel, R.E., in his absence from India, had temporarily placed at my disposal; and the project meeting with liberal support from Col. J. T. Walker, R.E., Superintendent, Great Trigonometrical Survey of India, I was enabled, through his kindness, to provide myself with four chronometers, a good altazimuth, a barometer, thermometers, and other articles of equipment necessary for the undertaking. My especial object in view was to observe the transit from *a considerable height*; and this condition was easily secured through the circumstance that I was located only 14 miles from Mussoorie, on the Himalaya Mountains. No doubt a station on these mountains would be very liable to an envelope of mist and cloud at the time of year in question; but, on the other hand, were really good weather to prevail, I should enjoy the advantages of an exquisitely clear atmosphere, such as I have never experienced save on the Himalayas. Add to this, the journey, as already stated, was merely an ordinary ride, the necessary equipment for my purpose was at hand, and though failure would involve a waste of no inconsiderable preliminary labour, this latter I was willing to incur if need be. Arguing thus, I selected a station some 6500 feet above the sea, and proceeded to find my latitude, longitude, and height, to observe for time, and to rate my chronometers. My numerical results will be communicated very shortly in a second note. The remarks here made are restricted chiefly to what I *saw* with the equatoreal.

The telescope of the equatoreal has a 5-inch object-glass, with about 60 inches focal length, and is driven by an excellent clock. The eye-end may be fitted at will with an eyepiece of 55, 85, 125, 200, or 300 power,

or with a spectroscope mounting a single simple prism. The polar axis may be shifted for latitude. The equatoreal was set up and adjusted in an observatory-tent, of which the canvas top was removed during observation.

I found from actual trial that the most suitable eyepiece for both ingress (sun's altitude 2° 24′ to 7° 29′) and egress (sun's altitude about 26°) was that of 125 power; accordingly *this* eyepiece alone was employed *at* the contacts. It was, however, impossible to adopt the same dark glass for both the higher and lower altitudes, without sacrificing definition on one of the two occasions. Accordingly I selected for ingress two glasses which, combined, gave a neutral or bluish field; and for egress I changed one of these for a deep-red glass, so that the field now presented a moderately deep red. The glasses were quite flat, and lay against one another in intimate contact, giving excellent definition. I may here add that, thanks to Manrakan, artificer, G. T. Survey, the clock behaved with perfect regularity; nor was there the smallest instrumental disappointment throughout the work. As regards procedure during transit, my friend Mr. W. H. Cole, M.A., counted seconds audibly from the journeyman chronometer placed before him; while Baboo Cally Mohun Ghose, computer, took up a position by my side, noting down such remarks as the phenomena, viewed through the equatoreal, elicited from me. Nor, as we stood informed, were the events to be recorded few in number. At, for instance, ingress, internal contact, the light between the cusps was to vary suddenly; the cusps were to meet. Then came "the pear-drop," "the ligament;" what should be its length, what its breadth in terms of Venus's diameter, what its shape? when would it break? and, lastly, which of the known descriptions would it resemble? Primed on these points, we settled on a programme which should include them all, and time after time we went through the necessary rehearsals for perfecting ourselves in our parts. So much for what we had to expect, and now for a few words on the weather.

I moved up to my mountain-station on the afternoon of December 1. The day following I was busy fixing my station, adjusting instruments, &c. Up to this the sky had been sufficiently clear; but from the 3rd there set in an alternation of weather, which, without running into extremes, kept me in a state of miserable suspense, to say nothing of the additional watching and toil in observing for time. This state of affairs appeared drawing to a climax, when the 8th December arrived, and the clouds looked blacker, while the mist, which generally precedes snowfalls here, began as usual to settle down on the internal and still more lofty ranges of hills. About 2 P.M. on this day there were a few drops of rain or sleet, so few that they might almost have been counted; and later on, as the sun began to set, the heavy cumuli evolved themselves into strati, which, spreading their even canopy over us, left something like decimal 0 of sky visible; and this at 10 o'clock at night! At 4 next morning, Dec. 9,

when again reported on, the sky was without speck or cloud; the air was still, and so clear, so brilliantly clear, that even the most exacting of observers could not have desired more favourable circumstances for viewing the coming transit. In brief, I enjoyed most exquisitely clear weather during my observations—such weather as occurs not frequently even at Mussoorie.

I now proceed to describe the phenomena of the transit. In doing so I shall have occasion to speak of Venus as she appeared *across* the sun's limb, when one portion of her own limb is seen against the sun, and the other remains against the sky. The former portion I shall call Venus's sun-limb, or V_n, the latter Venus's sky-limb, or V_k. Again, I shall require to mention a ring of light around V_k, which I shall indicate by L_k, the corresponding ring around V_n being understood by L_n. Another point is this: any one who has watched, say the sun's limb, especially at a low altitude and with high power, must be aware of the turmoil or ebullition which there appears, very much as if the limb was being boiled. I shall denote this kind of turmoil by "boiling."

Ingress.—With the telescope well and carefully adjusted for focus, I watched for the coming first external contact, but to no purpose; for I did not detect Venus's limb until after it had made an indentation on the sun's limb. The latter boiled sensibly, but by no means violently. It appeared jagged, and as if with minute spikes projecting inwards, all of which were well defined in the bluish field. Watching V_n, I found it also boiling slightly, but in a manner somewhat different to the sun's limb. The appearance was that of boiling vapour coming round from the face of Venus, turned towards the sun and overlapping V_n; moreover this boiling was not restricted to the edge of V_n, but extended $2''$ or $3''$ beyond, thus forming a kind of boiling annulus*, in which there were minute sparkling specks dancing and shifting about, appearing and disappearing; the edge V_n was seen through the boiling. So much for that portion of Venus seen against the *sun*.

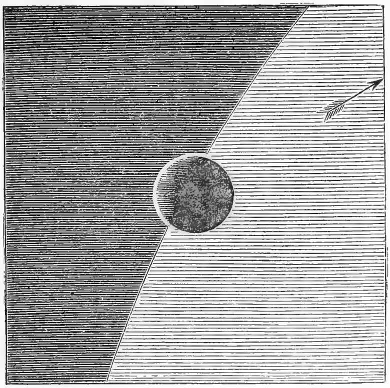
Ingress (continued).—At $8\frac{1}{2}$ minutes before the first internal contact took place, I happened to look closely into the spot where that part of Venus against the sky lay; and, to my great surprise, I found that this portion of her disk was easily visible, because it was edged by a narrow ring of light, or L_k. At first I saw L_k for only about $10°$ or $15°$ on either side of the point where the chord of Venus's track would cut V_k—or, more definitely stated, this light-ring did not reach either way to the sun's limb; but within the next 50^s I saw the ring distinctly round the whole sector of V_k, from edge to edge of the sun's limb, as shown in the rough sketch † accompanying. The light-ring was only moderately bright, like

* This cannot be shown in the sketch accompanying, for its presence became visible chiefly through the *movements* taking place.

† I must apologize for the roughness of my sketch. I am, however, writing against time to catch the outgoing mail; and of drawing-materials I possess nothing more at

diffused light; and at first I estimated this annulus at 3″ in breadth. It was probably brightest about the point where the chord above named cut

Ingress.



The annular ring of light could be distinguished, in continuation, around the limb *on the sun*; but as this continuation was rendered visible chiefly by the *movements* taking place in it, it is necessarily absent here.

V_k. At 6^m before first internal contact, I estimated L_k at 4″ in breadth; at the same time my notes contain the remark, "definition excellent." At 3^m 24^s before first internal contact I remarked, "light-ring quite distinct;" and 1^m 16^s later I stated, "light-ring quite bright." Indeed the annulus L_k was so plain that, after recording the time of transit for the *dark* edge V_k, I even made a conjectural record as to the time when the *bright* edge (L_k) transited across the sun's limb. Of course this latter estimate was based on *recollection* of the width of L_k, and not on any *visible* fact; for as L_k came on the sun's disk, the lesser light of the former merged into the greater light of the latter. On the whole, I am of opinion that L_k was between 2″ and less than 4″ in breadth.

Ingress (continued).—"And now for 'the pear-drop,' 'the ligament,' &c.," I mentally exclaimed, as I watched the following limb of Venus which had just transited. From what has been stated it will be seen that as V_k passed onwards from the sun's limb, it was followed immediately and *visibly* by the light-ring L_k; so that, unless Venus *suddenly shot backwards across this ring of light*, there could be no "pear-drop" and no "ligament." Fully expecting this retrogression, I still watched intently for the event, while my friend Mr. Cole went on deliberately enumerating seconds amid the complete silence enforced on all others around;

present than the stump of a broken blacklead pencil and a blue-red carpenter's pencil. A better sketch will be forwarded hereafter. [A woodcut of the better sketch will accompany Note No. 2.—G. G. S.]

but I watched for any such event in vain. Venus glided resolutely onwards, and the streak of *light* she was leaving behind grew wider and still wider, until at last, when a belt of light representing 10 or 15 minutes lay between her and the sun's limb, I exclaimed (and I believe much to the disappointment of all concerned), "*there is no pear-drop and no ligament.*" We watched Venus for some half an hour after this, and then turned to the spectroscope.

The Spectroscope.—The substitution of spectroscope for eyepiece cannot be effected in this instrument without a considerable amount of adjustment. When this had been performed, and we had taken a hasty breakfast, I first placed the slit across the *centre* of Venus's disk, and found that it gave a black band all along the length of the bright solar spectrum, *i. e.* Venus's face turned to us reflected no light. I next placed the slit tangential to Venus's disk. This gave a faint glimmer of narrow light in place of the black band, *i. e.* this glimmer was slightly brighter than the solar spectrum on which it appeared. I looked intently for Venus's air-lines; but, so far as the feeble dispersion of the prism would show, the lines seen across the glimmer from Venus's edge were identical in all respects with the solar lines. I repeated these experiments as long as time permitted, and then, with artificer Manrakan's help, reverted to the adjustments for mounting the eye-end and eyepiece employed at ingress. As already stated, it was now necessary to substitute a dark-red glass in place of one of those used at ingress.

Egress.—The field now was red, and, if any thing, slightly too dark to show faint light; the definition was intensely sharp. Again I watched with the utmost care as Venus approached her second internal contact; *but neither pear-drop nor ligament, nor any other connexion or shadow, appeared between her limb and that of the sun, until at last the two fairly touched**. Of course I again looked for the light-ring around Venus's sky-limb; and I certainly saw it, though faintly, 5^m after the above contact. Some time later, when I once more searched for the light-ring, it was invisible, nor did I again see it, nor yet the disk of Venus, against the sky.

Before concluding this note, I take the liberty of adding a few words, of no little importance, from the only letter on the transit which has as yet reached me. It is from Colonel Walker, who was at Dehra Doon, in the valley below, some 10 miles south of my position, and at a height of about 2200 feet. He observed the phenomena of the transit with a very tolerable telescope, by Solomons (I believe), of, I fancy, some 3-inch object-glass, and perhaps $3\frac{1}{2}$ feet focal length. The Colonel writes, "We saw the pear-drop and the ligament very distinctly."

I conclude from what has preceded that :—

* My notes state successively that when the two limbs were apart $\frac{1}{4}$, $\frac{1}{6}$, $\frac{1}{8}$, $\frac{1}{12}$, and $\frac{1}{20}$ of Venus's diameter, there was no ligament or other connexion between them.

(1) In view of the light-ring L_k, and of the peculiar boiling annulus around V_n, which may be called L_n, I have no doubt that L_n was, in fact, a continuation of the light-ring L_k, *which latter, beyond all question, was plainly visible*; and under these circumstances it may be urged that Venus is surrounded by an atmosphere which at the time was made *visible* to the extent of 2″ to under 4″ in breadth.

(2) As a matter of fact, the pear-drop and the ligament were visible at a height of 2200 feet, but at 6500 feet the ligament was invisible. The influence generally of height of station, from this evidence, appears undeniable; but the phenomenon still remains to be accounted for definitely. If, however, an effective atmosphere of x breadth around Venus be conceded, this atmosphere may be supposed to stop a certain amount of direct light from the sun, producing a slight shade around Venus corresponding to the breadth x. This shade would, I conceive, be quite invisible when its outer edge is backed by the sun's bright light; but could we contract the sun to a diameter equal to that of Venus *plus* twice x, and make Venus and the sun concentric, it appears likely that we should see a shaded annulus right round Venus between her limb and that of the sun; further, that the annulus would appear darker at low than at higher altitudes, and would become invisible when the observer was raised above a sufficiency of the earth's atmosphere. Should these suggestions prove tenable, the ligament seen would break when the outer edge of the shade, corresponding to x, transited across the sun's limb.

(3) Solar light shining through Venus's atmosphere, if any, produces no alteration in the lines of the solar spectrum, so far as the dispersion of a single simple prism can show. Also Venus's face, turned towards us, reflects no light during transit, subject to the same instrumental test.

Night of 10th Dec., 1874.

III. "Appendix to Note, dated November 1873, on White Lines in the Solar Spectrum." By J. B. N. Hennessey, F.R.A.S. Communicated by Professor Stokes, Sec.R.S. Received January 11, 1875.

After detection of the white lines 1650 and 1658 (Kirchhoff's scale) at Mussoorie in November 1873, I discovered two other such lines before leaving that station of observation, viz. 2009 and 2068 (about). On 20th November, 1873, I packed up the spectroscope, taking particular care that the prisms should not shift from the position they then occupied.

On 28th November, 1873, I set up the spectroscope in the Dome Observatory at Dehra, in the valley below, the prisms retaining their former position, and my recollection of the white lines seen at Mussoorie

being still quite vivid. I now found that 1650 and 1658 were distinctly seen; but they were no longer nearly of the pure white colour they presented at the higher station, while what may be termed the gloss about their whiteness, which induced me to describe them as resembling "threads of white silk held in the light," had quite disappeared; indeed they were now so decidedly greenish as not to invite attention. White line 2068 I now could hardly see, and 2009 was invisible, notwithstanding that I was quite familiar with the positions they occupied, and had made careful notes on the subject.

After this I released the prisms and turned them about variously, without producing any alteration in the white lines as they were now seen.

The height of the spectroscope above sea-level was

at Mussoorie	7100	feet.
„ Dehra	2200	„

February 18, 1875.

JOSEPH DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

Pursuant to notice, the Right Hon. Sir Stafford H. Northcote, Bart., C.B., Chancellor of the Exchequer, was balloted for and elected a Fellow of the Society.

The following Papers were read:—

I. "On the Number of Figures in the Reciprocal of each Prime Number between 30,000 and 40,000." By WILLIAM SHANKS. Communicated by the Rev. Dr. SALMON, F.R.S. Received January 5, 1875.

The further extension of my previous Table III. has enabled me to add "26" (see "Determination of a Prime Number," Proc. Roy. Soc. June 18, 1874) to the list of complete resolutions; for the factor 10583 13049 is smaller than 40000^2, and is therefore a prime number. "99" in the same Table may now have the large factor somewhat reduced, and stand as follows, since $34849 \equiv 99$:—

99 | 199 . 397 . 34849 . 36321 69409 21057 80278 45603 26475 97861 29249 67984 25182 29368 83.

In Table III., from 20,000 to 30,000, the following *corrigenda* are required:—

		Pro	*lege*
Opposite	20071	6690	3345
"	20143	10071	20142
"	20353	20352	6784
"	20359	20358	10179
"	20939	20938	10469
"	21277	1181	1182
"	21821	10910	21820
"	23599	874	437
"	25667	25666	12833
"	25759	25758	12879
"	27427	27426	13713
"	27739	13869	27738
"	28663	4777	9554
"	28687	14343	28686
"	28751	1150	575
"	28843	14421	759
"	29443	29442	14721
"	29527	777	1554

Between 22003 and 22027 insert 22013, and opposite to it 5503.
" 22961 " 22973 " 22963, " " 11481.
" 28933 " 28961 " 28949, " " 28948.
" 29383 " 29389 " 29387, " " 2099.

Note.—I have been kindly and ably assisted by the Rev. Prof. Salmon, F.R.S., in *revising* the Table from 20,000 to 30,000, also in *calculating* and *revising* the Table from 30,000 to 40,000.—W. S.

[The Table from 30,000 to 40,000 is preserved for reference in the Archives of the Society, by order of the Committee of Papers.—G. G. S.]

II. "On the Nature and Physiological Action of the *Crotalus*-poison as compared with that of *Naja tripudians* and other Indian Venomous Snakes; also Investigations into the Nature of the Influence of *Naja*- and *Crotalus*-poison on Ciliary and Amœboid Action and on *Vallisneria*, and on the Influence of Inspiration of pure Oxygen on Poisoned Animals." By T. Lauder Brunton, M.D., F.R.S., Sc.D., M.R.C.P., and J. Fayrer, C.S.I., M.D., F.R.C.P. Lond., F.R.S.E., President of the Medical Board at the India Office. Received January 7, 1875.

In our former papers we described the general phenomena accompanying the physiological action of cobra- and *Daboia*-poisons on warm-blooded animals, reptiles, fishes, and invertebrata. We propose in this

paper to compare with these the action of the *Crotalus*-virus in its general effects on life, on the functions, organs, and tissues, and especially as it affects the blood and vessels as regards a marked influence in causing hæmorrhages and extravasations of blood generally and locally; and, further, to examine the action of snake-poison generally on ciliary and amœboid movements—or that which represents its action on contractility, apart from that which is caused through the medium of the nerve-centres and nerve-distribution.

It appears that there is little difference between the physiological effects of the crotaline or viperine and the colubrine virus. The mode in which death is brought about is essentially the same in all; though there are evidences, even when allowing for individual peculiarities, that the action is marked by some points of difference sufficiently characteristic to require notice in detail.

We have already expressed our belief that death is caused by the cobra-, *Daboia*-, and *Hydrophis*-poison, 1st, through its action on the cerebro-spinal nerve-centres, especially on the medulla, inducing paralysis of respiration; or 2nd, in some cases (where the poison has entered the circulation in large quantities and has been conveyed more directly to the heart) by arrest, tetanically in systole, of cardiac action, probably owing to some action on the cardiac ganglia; 3rd, by a combination of the two previous causes; 4th, by a septic condition of a secondary nature, and which, being more essentially pathological in its bearings, the details were not considered suitable for discussion here.

There is reason to believe that death is caused in the same way by the *Crotalus*-poison also; and it appears, from the experiments recently performed in Calcutta, by Dr. Ewart and the members of the Committee appointed by Government, upon *Pseudechis porphyriacus*, or the black snake, and *Hoplocephalus curtus*, or the tiger-snake of Australia, that their virus causes death in the same manner. These reptiles had been sent from Melbourne to Calcutta for the purpose of investigation and comparison. (*Vide* Committee's Report, p. 58 *et seq.*, Appendix.)

But though the actual cause of death is essentially the same, the phenomena which precede and accompany it differ in some degree according to the nature of the poison, the quantity and site of the inoculations, and the individual peculiarities of the creature inoculated, as may be seen in the experiments herewith recorded.

The condition of an animal poisoned by the rattlesnake-venom, then, essentially resembles that of one subjected to the influence of the colubrine or viperine poison of Indian snakes:—

Depression, hurried respiration, exhaustion, lethargy, unconsciousness, nausea, retching, and vomiting (*vide* experiment on cat, Exp. IX.).

Muscular twitchings, ataxy, paralysis, and convulsions (the latter probably chiefly, though not entirely, due to circulation of imperfectly oxygenated blood, the result of impeded respiration), and, finally, death.

Hæmorrhages or hæmorrhagic extravasations and effusions, both local and general, occur in all varieties of snake-poisoning.

But we observe (and in this our observations are in accord with those of Weir Mitchell) that there is a greater tendency to both local and general hæmorrhage and extravasation of blood and of the colouring-matter of the blood, especially as observed in the peritoneum, intestines, and mesentery, and also probably to a more direct action on the cord (*vide* Experiments I., III., V., VI., VII., IX., XI., XIV., XV.), than in poisoning by either cobra or viper (*vide* Experiments IV., VII., XIII., XVI., XVII., XX.).

The viscera and other tissues, after death, are found congested and ecchymosed, and in some cases to a great extent, seeming to show that either a preternatural fluidity of blood or some important change in the vessels, favouring its exudation, has occurred.

But with regard to the blood itself we have observed that it does form a coagulum after death, generally, if not invariably; as we have noted to be the case, though not to the same extent, in the blood of animals that have succumbed to the *Daboia*-virus*.

With reference to the coagulation or non-coagulation of the blood in cases of snake-poisoning, we observe that the following conclusions have been arrived at by Mr. Richards and the Calcutta Committee (*vide* p. 45 of their Report).

"We now propose to deal with the physical changes produced by snake-poisoning on the blood. From observations which have been made by Mr. Richards and ourselves, we have arrived at the following conclusions.

"*The blood appears to remain fluid after death under the circumstances noted below*:—

"1st. When a large quantity of the cobra-poison has been directly injected into the circulation, as, for example, into an artery or a vein†.

"2nd. In cases where animals or men have been poisoned by the bite of vipers, such as the Russell's viper.

"3rd. In all cases of snake-bite, whether from the poisonous colubrine or viperine genera, in the human subject‡.

"*The blood undergoes either partial or complete coagulation under the following conditions*:—

"1st. When a small quantity only of the cobra-poison has been injected into a vein or an artery.

"2nd. In cases where the lower animals have been bitten by the cobra.

"Why the admixture of a large and quickly fatal injection of the cobra-virus into the circulation of animals should produce comparatively

* In Dr. Fayrer's Indian experiments the blood of animals dead from *Daboia*-poison nearly always remained fluid after death.

† This is not always so.—J. Fayrer.

‡ Not always so.—J. Fayrer.

permanent fluidity of the blood or interfere with its ordinary coagulability soon after removal from the body or after death, and why the injection of a smaller and more slowly fatal quantity should interpose no obstacle to its speedy coagulation, are questions extremely difficult to account for or explain. We can only state the fact that, in the one case, coagulation occurs speedily, and in the other this coagulation is retarded or altogether prevented by some cause at present unknown."

The following experiments were made on the physiological action of the virus of the rattlesnake, with the view of comparison with that of the cobra and *Daboia*.

We are indebted to Dr. Weir Mitchell, of Philadelphia, for a supply of the virus. He was good enough to send about six grains of the dried poison of *Crotalus*—the species not named, but it is believed to be of *Crotalus durissus*.

The dried poison supplied is said to be about 6½ years old, and was dried in July or August at the natural temperature, and has since then been preserved in a phial. It was tried by Dr. Mitchell and found active three years ago.

It has the appearance of fractured fragments of dried gum-arabic, of rather a darker yellow colour, but otherwise resembling the dried cobra-virus sent from Bengal.

Experiment I.

June 9th, 1874.—·015 gramme of the dried *Crotalus*-poison diluted with 1 cub. centim. of distilled water was hypodermically injected into the thigh of a full-grown guineapig at 11.30 A.M.

Restlessness and muscular twitchings of the body generally soon commenced; these passed away, but the animal became sluggish, in which condition it remained all night, and died at about 9 A.M. the next morning.

The injected limb became much swollen, infiltrated, and discoloured with sanguineo-serous effusion.

The intestines were not ecchymosed; there was much sanguinolent fluid and also blood effused into the abdominal areolar tissue.

No convulsions were observed; but as the animal was not seen during a short time previous to death, they cannot be said positively not to have occurred; nor is it known if the heart ceased to beat at the moment when apparent death took place.

Experiment II.

A few drops of watery solution of *Crotalus*-poison, of same strength, were injected under the skin of a guineapig's thigh at 12.16, noon.

12.17. Marked twitchings of head and hind legs, very similar to those produced in some of the cases of cobra-poisoning.

12.18. Hind leg (poisoned one) weak.

12.20. Twitchings much increased, now mainly in head and neck, not so much in hind legs.

12.28. Guineapig quiet, but with occasional twitchings; sluggish and disinclined to move.

1.30. Sluggish in moving; can still move about, though disinclined to do so. The punctured thigh is very blue.

The rest of the notes of this experiment were lost.

The animal died.

Experiment III.

June 10*th*, 1874.—$\frac{1}{4}$ of a grain of *Crotalus*- and $\frac{1}{4}$ of a grain of cobra-poison were carefully weighed and diluted, each with ten drops of distilled water. Two full-grown guineapigs of equal weight were then selected.

The solution of *Crotalus*-poison was injected into the peritoneal cavity of guineapig No. 1 at 1.52 P.M.

1.55. Muscular twitchings of head and neck.

2 P.M. Startings and twitchings continue.

It gives faint squeaks occasionally, as though the sudden startings which occur at intervals of 5 or 6 seconds cause pain.

2.5. Twitchings continue.

2.8. Very restless; twitchings going on, but no paralysis yet.

2.17. The same.

2.25. Restless and weaker; but still moves freely on being roused.

2.42. Sluggish; drags the hind legs.

2.58. Weaker; rolls partially over on one side, but can run when roused.

3.3. Lying on side, but can be roused; is partially paralyzed in hind legs. Respiration abdominal and hurried.

3.5. Nearly quite paralyzed; is roused with difficulty.

3.7. Can still be roused. Abdomen distended and painful; cries out when it is touched, as though peritonitis were setting in.

3.12. Can be roused with difficulty; respiration hurried; convulsive movements of fore legs and neck. Can still stagger for a few paces; but coordination of muscular power much diminished.

3.30. In violent convulsions.

3.38. Convulsions continue.

3.45. Quiet. Paralyzed; but reflex action still continues.

3.55. Dead in 2 hours and 3 minutes.

3.56. Electrodes in cord cause twitching of muscles of the back, and very slightly in those of the legs: the cord was evidently all but paralyzed. Muscular fibre contracts freely to direct stimulus of current. The intestines were ecchymosed and congested. There were effusions of red serum into the peritoneal cavity, and much ecchymosis of peritoneum and subperitoneal and intra-muscular areolar tissue. Peristaltic action continued faintly.

4 P.M. The heart has ceased to contract 4 minutes after apparent death; it continued to contract, especially the auricles, for part (not the whole) of the time.

The blood removed from the heart-cavities and vena cava rapidly formed a firm coagulum in a glass receiver.

The electrodes applied to the sciatic showed that the nerve-trunk, as well as the spinal cord, was paralyzed.

Experiment IV.

Guineapig No. 2, an albino, had the $\frac{1}{4}$-grain cobra-virus solution injected into its peritoneal cavity at 1.56 P.M.

It immediately became much excited.

1.57. Is now quite tranquil.

2. Sluggish. Does not twitch as guineapig No. 1 did.

2.4. Started and squeaked slightly, as though in pain, but no twitching.

2.5. Slight twitching generally. Paralysis and ataxy commencing; drags its legs with difficulty.

2.9. Sharp twitchings of head and neck.

2.12. Subsided on to the belly; head fallen over; crawls with difficulty; is very feeble, almost paralyzed. The albino eyes have a heavy dull look; lost their bright pink.

2.14. Convulsed.

2.15. Reflex action ceased. Apparently dead, but heart can still be felt beating. Occasional convulsive twitchings of lower lip.

2.16. Dead in 21 minutes.

2.17. All movements have ceased. Heart had ceased to contract, except slight flickering movements of auricles.

2.20. Electrodes in cord. Spinal cord and nerves paralyzed; muscles contract freely to direct stimulus of current. Heart distended with blood. Blood, when removed, formed rapidly a firm coagulum. Intestines, peritoneum, and subperitoneal areolar tissue congested and ecchymosed. Sanguinolent effusions into peritoneum, but not so well marked as in the *Crotalus*-poisoning. Peristaltic action of bowels ceased rapidly.

The results of these two experiments show, so far, that the action of the cobra-poison is more energetic than that of the rattlesnake. Both were watery solutions of exactly the same quantity of the dried virus; but it is to be borne in mind that that of the rattlesnake was $6\frac{1}{2}$ years old, while that of the cobra was only one year old.

The guineapigs were both full-grown and of the same size; yet one succumbed in 20 minutes to the cobra-poison, while the other survived the inoculation of the rattlesnake-poison for 2 hours and 3 minutes.

There were no very marked differences in the action of the poison in

these two cases, except in the energy with which the cobra exceeded the *Crotalus*.

Crotalus.	*Cobra.*
Twitchings; restless; squeaks; sluggish; ataxy; paralysis. Hurried respiration. Peritonitis. Convulsions. Death in 2 hours 3 minutes. Coagulated blood. Ecchymosis and extravasation of serous effusion well marked. Cord paralyzed. Muscles retain irritability.	Twitchings; excitement; squeaks; sluggish; ataxy; weakness; paralysis. Convulsions. Death in 20 minutes. Spinal cord and nerves paralyzed. Muscles irritable. Heart distended. Blood congested. Ecchymosis. Congestion less than in *Crotalus*.

Experiment V.

June 10*th*.—A grain of *Crotalus*-poison diluted with water was injected into the peritoneum of a full-grown guineapig at 2.40 P.M. Twitchings began almost immediately.

3.3. Restless; startings; staggers on hind legs.

3.20. Very weak, especially in hind quarters. General paralysis setting in. Abdomen distended and very tender.

3.30. In convulsions. Still feels when the abdomen is touched.

3.37. Paralyzed; but feels the touch. Reflex well marked.

3.45. Apparently dead in 65 minutes.

3.48. Cavities opened. Auricles flickering. Blood from heart and great vessels coagulated firmly. Abdominal cavity and areolar tissue and subperitoneal tissue infiltrated with bloody serum. Much ecchymosis of peritoneum and intestines, but not of lungs. Cord and nerves paralyzed. Muscles contract vigorously to induced current.

Action of Crotalus-*poison on Rabbit.*

Experiment VI.

$\frac{1}{4}$ of a grain (·015 gramme) of the same *Crotalus*-poison, dissolved in 1 cub. centim. of water.

The jugular vein of a large white rabbit was exposed, and the above solution was injected into it at 1.50 P.M.

At 1.51 violent convulsions, with opisthotonos.

At 1.53 apparently quite dead. Artificial respiration commenced immediately. Heart acting still, though feebly and with irregular flickering contractions. Spinal cord exposed. Electrodes applied; no reaction.

2.12. Heart still contracting feebly.

2.15. Faint contractions of heart still observable. Ventricles punctured, and blood withdrawn. Peristaltic action has ceased.

2.20. Feeble cardiac movements continue.

2.21. Heart has now ceased. Muscles react to direct current. Death

caused by rapid paralysis of medulla and cord. The blood taken from the heart and great vessels did not coagulate. At 4 P.M. it was still fluid, though very florid in colour.

Examined under the microscope nearly 2 hours after apparent death, the white corpuscles appeared natural; the red corpuscles not in rouleaux, and very much crenated, though a few retained their natural contour.

The blood was natural to test-paper.

Experiment VII.

June 17*th*.—$\frac{1}{4}$ of a grain (·015 gramme) of dried cobra-poison, dissolved in 1 cub. centim. of water, was injected into the jugular vein of a large white rabbit, of the same size as in the previous experiment, at 2.55 P.M.

The rabbit passed at once into violent convulsions, and was apparently dead before it could be removed from the board, within one minute. The cord was immediately exposed, artificial respiration having also been begun. Electrodes applied, with strong current; no reaction; the cord was perfectly paralyzed.

Thorax examined at 2.59. Heart had ceased to contract. Ventricles moderately contracted. Auricles distended with blood. Phrenic irritated, quite paralyzed. Diaphragm, when directly irritated by current, contracts very faintly, whilst the neighbouring muscles contract vigorously. Peristaltic action goes on. Electrodes applied to vagus appear to accelerate peristaltic action; applied to splanchnic, they diminish it.

3.7. Ventricles of heart have now contracted firmly.

3.15. Blood taken from heart and great vessels has coagulated, but not firmly. The clot is small, and the serum very red.

3.15. Electrodes to sciatic; no reaction. Blood examined under microscope; no aggregation in rouleaux, no crenation of corpuscles. Blood neutral to test-paper.

We have in former papers remarked that when the cobra-poison was injected into the jugular vein directly and caused almost immediate death, that the fatal result was due to cessation of the heart's action by arrest in systole, and such was partially the case in the last experiment (VII.), made for the purpose of comparison with *Crotalus*; but in experiment VI. death was not so caused, for the heart continued to contract for about 28 minutes after apparent death, which was probably due to the sudden and total annihilation of the functions of the medulla and cord, no reaction to a strong current occurring when the electrodes were applied immediately after apparent death.

In this instance of *Crotalus*-poisoning it is also to be remarked that the coagulability of the blood was destroyed, whilst in that by cobra-virus it was only partially so.

It appears from the results of this experiment that the direct inoculation of large doses of the virus, whether viperine or colubrine, into the

circulation have the power in some cases of annihilating almost instantaneously the irritability of the cord and medulla, as in others they have of arresting the heart's action.

Experiment VIII.

June 17th.—Ten drops of the blood of the rabbit described in the last experiment, poisoned by *Crotalus*-virus, were injected into a guineapig's thigh at 3.40 P.M.

The guineapig was not apparently affected constitutionally by the poisoned blood. It was alive the next morning; but the leg was swollen and discoloured. It ultimately recovered.

Experiment IX.

June 24th, 1874.—A full-grown cat was chloralized at 1.20 P.M. $\frac{1}{4}$ of a grain of *Crotalus*-poison, diluted with 1 cub. centim. of water, was injected into the jugular vein. The respirations were immediately quickened.

1.21. Twitching of muscles generally.

1.22. Efforts to vomit. Forcible extension of limbs.

1.24. Hurried respiration and retching. Reflex action perfect.

1.30. Muscular twitching and tetanic stretching of limbs. Efforts to vomit continue. Micturition. Rolls over on the ground.

1.34. Ataxy. Staggers when walking, which it can only do for a few paces. Peculiar twitching of diaphragm; not synchronous with respiratory movements. Rolls over on its side.

2 P.M. In the same state.

2.8. Injected $\frac{1}{8}$ of a grain more of the poison into the same jugular vein. The animal immediately got up and walked, comparatively steadily, for several paces, as though it had been stimulated, and then rolled over.

2.16. Twitching of diaphragm continues at the rate of 150 per minute.

2.18. Again got up and walked for a few paces; but it is gradually becoming more paralyzed.

2.44. Violent tetanic spasms of limbs. Reflex action diminished.

2.46. Reflex action gone from eyes. Deep sighing respiration.

2.47. Convulsions. Death. Body opened immediately. Lungs deeply congested and much ecchymosed. Deep red gelatinous effusion all about the roots of the lungs. Heart contracting. Electrodes applied to phrenic caused vigorous contraction of diaphragm.

2.50. Heart ceased to contract 3 minutes after respiration had ceased.

2.52. Electrodes in cord; do not cause contraction of limbs.

2.54. The sciatic nerve, when irritated, conveys impressions; muscles of legs contract. Blood from the heart and great vessels did not form a coagulum, and remained permanently fluid. Red corpuscles of blood were much crenated.

Death in this case appeared to be caused through the medulla.

Experiment X.

June 15*th*, 1874.—Action of *Crotalus*-poison on the frog.

A frog's hind leg was ligatured excluding the sciatic nerve.

A solution of *Crotalus*-poison was injected into the lymph-sac at 12.32 P.M.

2.30. Sluggish, but not otherwise affected.

3.15. In the same condition.

June 16*th*.—12.3, noon. Sluggish, but can still move.

June 17*th*.—Found dead this morning early; pupils contracted.

Electrodes applied; no reaction in either cord or nerves on either side to the strongest current.

The frog may have been dead some hours.

Experiment XI.

June 15*th*.—At 3 P.M. same day a solution of *Crotalus*-venom was injected into the dorsal lymph-sac of a frog, the aorta having been previously ligatured, so as to prevent the poison from affecting the trunks or peripheral extremities of the sciatic nerves.

3.40. The frog seems quite unaffected.

June 16*th*.—12.30, noon. Frog dead; not rigid; mouth open.

Irritation of cord with strongest current does not cause contraction of legs. Irritation of sciatic with coil at 24 causes twitchings of gastrocnemius.

Neither of these two experiments give any definite results, as the period intervening between death and examination of the condition of the nerve-centres was not determined exactly.

The results of the following experiments show that the local as well as the general effect of the cobra- and *Crotalus*-poisons, *i. e.* colubrine and viperine, is to cause hæmorrhage, ecchymosis, and sanguinolent effusions into the areolar tissue, not only at the seat of inoculation and its neighbourhood, but also in the mucous membranes and other vascular parts. It is obvious also that the *Crotalus*-poison acts more energetically in this respect than the cobra-poison, and that this is perhaps one of the most marked distinctions between them.

Experiment XII.

August 6*th*, 1874.—A cat was chloralized, and part of the mesentery placed under the microscope on the warm stage. *Crotalus*-poison, diluted with water, was then applied to the mesentery, and its effects watched. The white corpuscles were observed to cling in quantities to the walls of the vessels, and as the current of blood hurried through them, some masses of pale matter, like aggregation of white corpuscles, were observed to pass with the stream; very soon, marked extravasation of red corpuscles took

place, and to the naked eye the mesentery became discoloured by patches of ecchymosis in the course of the small blood-vessels, like the foliage on the branches of a tree.

There could be no doubt that the local action of the poison had a marked effect in producing extravasation of blood.

Experiment XIII.

A similar experiment was repeated on another part of the mesentery of the same cat with cobra-poison, exactly as the *Crotalus*-poison had been applied in the previous experiment. This was carefully watched, but no extravasation took place; there was a marked difference in the result of the application of the two poisons, at all events as far as these two experiments were concerned.

Experiment XIV.

August 12*th*, 1874.—A cat was chloralized at 2.30 P.M. Mesentery exposed and placed under microscope on warm stage.

Crotalus-poison applied to mesentery; circulation soon diminished in some vessels but continued vigorously in others. Isolated extravasated patches soon made their appearance of a triangular form, others followed and coalesced with these until a network was formed in the course of the vessels all over the field. The extravasation soon became general, the circulation still continuing slowly.

Experiment XV.

A fresh portion of mesentery of same cat exposed. Intestines becoming cold and circulation now very languid.

Cobra-poison applied.

No apparent effect produced; but the circulation is very languid, indeed has almost ceased, so that the results of this experiment are not conclusive.

Experiment XIV.

August 14*th*, 1874.—A cat was chloralized, part of mesentery withdrawn, and placed under microscope on warm stage.

Dried cobra-poison dissolved in a salt solution, ·75 per cent., applied to the mesentery at 4.10 P.M.

4.14. Circulation is languid, almost ceased in some vessels.

4.18. Slight extravasation taking place where the poison has been in contact.

4.20. Extravasation rather more obvious.

4.35. Exposed another part of the mesentery; examined the state of the circulation before applying the poison. Blood flowing languidly.

Poison applied at 4.37; at first it seemed rather to accelerate the movement of the blood.

4.38. Circulation continues at same rate.
4.42. Same rate.
4.45. It becomes more languid.
4.48. Circulation has ceased, but yet there is no marked extravasation.

Experiment XVII.

Another portion of the same mesentery had cobra-poison applied, but after half an hour there was no sign of extravasation.

Experiment XVIII.

A fresh piece of mesentery exposed of same cat, and diluted *Crotalus*-poison applied at 4.52 P.M.

The circulation was rather languid at the time, and apparently became more languid.

At 4.58 no extravasation had taken place, the blood flowing very languidly.

5.15. Circulation still going on, but very slowly; no extravasation; it soon after ceased.

Experiment XIX.

At 5.20 P.M. a fresh portion of the mesentery was exposed; to one part cobra- and to the other *Crotalus*-poison was applied, and the effect was watched with the naked eye.

5.45. No extravasation visible.

At 6.15 P.M. slight extravasation equally visible on both.

Experiment XX.

August 25*th*, 1874.—At 2 P.M. a young cat was chloralized. The mesentery was drawn out and a part treated with cobra-poison, another part with *Crotalus*-poison.

At 5 P.M. On examination, that under the influence of the *Crotalus*-poison was found deeply congested and reddened with blood, extravasated in the course of the small vessels, forming a well-marked redness to the naked eye. Under the microscope the red corpuscles were seen in numbers outside the vessels. Circulation still going on vigorously. That part treated with cobra-poison was barely altered, but, on close examination, slight patches of extravasation were seen in the course of the vessels.

The difference was well marked between the two—the extravasation produced by *Crotalus*-venom being well marked, that by cobra-venom scarcely perceptible. In both cases the microscope showed red corpuscles outside the vessels.

These experiments show that *Crotalus*-poison causes hæmorrhage and hæmorrhagic effusions more than the cobra-poison does.

The following experiments were made, at the suggestion of Mr. Darwin,

with the object of testing the influence of snake-poison on ciliary action, especially in reference to its comparative action on vegetable protoplasm, as will be seen by his remarks.

Experiment XXI.

Influence of Cobra-poison on Ciliary Action.

June 29*th*, 1874.—Ciliated epithelium from the frog's mouth was treated with a solution of cobra-poison and examined under the microscope.

At 1.35 P.M., when examined, the action of the cilia was vigorous.

At 1.45 it was much diminished.

At 1.55 it had entirely ceased.

Experiment XXII.

Ciliated epithelium placed under microscope; one part was treated with water, the other with the poisoned solution.

At 2.10 P.M. ciliary motion vigorous in both, perhaps more so in that subjected to the poisoned solution.

2.18. Non-poisoned cilia active. Poisoned cilia very feeble.

2.20. Non-poisoned cilia still active. Poisoned cilia very feeble.

2.24. Non-poisoned cilia active. Poisoned cilia very languid.

2.30. Non-poisoned cilia still active. Poisoned cilia have entirely ceased to act.

It is evident from this that the poison first stimulates and then destroys the activity of the ciliary action.

Experiment XXIII.

August 14*th*.—Frog's blood placed in salt solution, ·75 per cent., at 1.25 P.M. on warm stage, and then subjected to the action of cobra-poison.

At first the amœboid movements of white corpuscles went on vigorously. At 2 P.M. they had ceased, or very nearly so, in all that appeared in the field.

2.30. All movement had entirely ceased. The red corpuscles seemed more flattened, the nucleus more visible, and the edges better defined, assuming a pointed and more oval form than usual.

Experiment XXIV.

August 25*th*, 1874.—Newts' blood examined under $\frac{1}{8}$ object-glass on hot stage, white corpuscles moving slowly. Cobra-poison applied, but no perceptible change observed.

The following communications were received from Mr. C. Darwin on the action of some of the same cobra-poison on vegetable protoplasm:—

"You will perhaps like to hear how it acted on *Drosera*. I made a solution of $\frac{1}{4}$ gr. to ʒij of water. A minute drop on a small pin's head

acted powerfully on several glands, more powerfully than the fresh poison from an adder's fang.

"I also immersed three leaves in 90 minims of the solution; the tentacles soon became inflated and the glands quite white, as if they had been placed in boiling water. I felt sure that the leaves were killed; but after 8 hours' immersion they were placed in water, and after about 48 hours reexpanded, showing that they were by no means killed. The most surprising circumstance is, that, after an immersion of 48 hours, the protoplasm in the cells was in unusually active movement. Now, can you inform me whether this poison, if diluted, arrests the movement of vibratile cilia?"

"I dissolved $\frac{1}{2}$ gr. [of cobra-poison] in ʒj of water, so that I was able to immerse two leaves. It acted as before, but more energetically; and I observed more clearly, this time, that the solution makes the secretion round the glands cloudy, which I have never before observed. But here comes the remarkable point; after an immersion of 48 hours, the protoplasm within the cells incessantly changes form, and I never saw it on any other occasion so active. Hence I cannot doubt that this poison is a stimulant to the protoplasm; and I shall be very curious to find out in your papers whether you have tried its action on the cilia and on the colourless corpuscles of the blood. If the poison does arrest their movement, it will show that there is a profound difference between the protoplasm of animals and of this plant. Therefore if you try any further experiments I hope that you will be so kind as to inform me of the results. I may add that I tried at first 1 gr. to the ℥j, as that is my standard strength for all substances.

"It is certainly very remarkable that the poison should act so differently on the cilia and on the protoplasm of *Drosera*. After the 48 hours' immersion, I placed the two leaves in water and they partially reexpanded. I thought that the whitened glands were perhaps killed; but those of one leaf which I tried with carbonate of ammonia absorbed it, and the protoplasm was affected in the usual manner. I am very much surprised at the action of the poison on the viscid secretion from the glands, which it coagulates into threads and bits of membrane, with much granular matter. Have you observed whether the poison affects in any marked manner mucus or other such secretions?"

Experiment XXV.

Action of Cobra-poison on Muscle.

June 29th, 1874.—A standard solution of cobra-poison, ·03 gramme to 4·6 cubic centims. of water, was prepared.

1.25 P.M. The gastrocnemius of a frog was separated and immersed in this solution in a watch-glass; it immediately contracted considerably.

1.30. The muscle contracts with current at 11.

1.45. The muscle has lost its irritability; does not respond to the strongest current.

Experiment XXVI.

At the same time (1.25 P.M.) the gastrocnemius from the other leg of the same frog immersed in water. Did not immediately contract like that placed in the poisoned solution.

1.30. Contracts strongly to current at 15 c. m. of Du Bois Reymond's coil, more than the poisoned muscle at 11, at the same moment.

1.45. Contracts distinctly at 11, whilst the poisoned muscle has lost all irritability.

From this it is evident that the poison first stimulates the muscular fibre to contract, but rapidly afterwards destroys its irritability.

Experiment XXVII.

The gastrocnemii of a frog were again treated in the same way as in the previous experiment, with precisely the same results.

June 28th.—Made several experiments with cobra-poison on ciliated epithelium of frog's mouth, and found that it at first accelerated, then destroyed, the action of the cilia.

Experiment XXVIII.

To test the effects of Cobra-poison, when swallowed, on the Frog.

June 24th, 1874.—At 2.25 P.M. about $\frac{1}{8}$ of a gr. of dried cobra-poison was passed down a frog's throat.

2.30. Frog making violent efforts to vomit. Gaping. Head thrown back tetanically.

2.34. Bloody mucus vomited with violent efforts *.

2.50. Moves with difficulty; is becoming paralyzed. Efforts to vomit continue.

3. Much the same.

3.5. Very weak; still tries to vomit.

3.10. Reflex action still well-marked.

3.15. Motor nerves apparently quite paralyzed.

3.20. Apparent death.

Artificial Respiration with pure Oxygen.

As life had been prolonged for many hours in snake-poisoning by artificial respiration with atmospheric air, it was thought expedient to ascertain if the more complete oxygenation by the undiluted gas would be more efficacious, as it seemed might be possible; accordingly the following experiment was made on the 24th April, 1874.

* This experiment is especially interesting, as showing that frogs do occasionally vomit, a fact which has been denied by some physiologists.

Experiment XXIX.

$\frac{1}{4}$ of a grain of dried cobra-poison dissolved in distilled water was injected into a rabbit with the hypodermic syringe.

Symptoms of poisoning were rapidly manifested. A tube had been previously introduced into the trachea, and respiration was commenced as soon as poisoning was manifest.

Artificial respiration, with oxygen contained in a large bag, was steadily continued for two hours, but with no better effect than in other similar cases where atmospheric air was used for the same purpose. At the expiration of two hours, apparent death had occurred; the heart continued to beat for about two minutes after the respiration ceased.

Beyond a very florid condition of the blood, there was no obvious difference between the effect of oxygen and that of common air. It did not indeed appear that, as far as the effects produced by the poison were concerned, it differed in its action from common air.

Experiment XXX.

November 1874.—A little cobra-poison, dissolved in water, was added to water containing some cells scraped from the mantle of a freshwater mussel. Among these was a large ciliated cell, which, before the addition of the poison, had been been moving slowly, although its cilia were moving actively. Immediately after the addition of the poison the cell began to spin round on its own axis with extraordinary rapidity. In about three or four minutes its motions began to be languid, the ciliary motion ceased, the cell itself elongated, contracted, and then slowly resumed its former shape and became perfectly motionless.

Experiment XXXI.

Water from the interior of a freshwater mussel, and containing two specimens of *Paramœcium* in active motion, was examined. They were rotating with great rapidity. A little cobra-poison diluted with water was added. Three minutes after the addition one was discovered with both the cilia and cell-body perfectly still. The cilia of the other were still, but the cell-body was contracted. In about half a minute more it expanded to its normal size and then remained perfectly still.

Experiment XXXII.

A piece taken from the mantle of a freshwater mussel was placed on the slide and examined at the end of about half an hour. Active ciliary motion could be observed both in the fringe of the mantle itself and in several specimens of *Paramœcium*. A little dilute poison was added. At first the ciliary motion seemed increased, but in about two minutes it became slower, and in six had become very languid, and in ten minutes stopped altogether in the specimens of *Paramœcium*, but still continued in some of the cilia of the mantle.

Experiment XXXIII.

A little dilute cobra-poison was added to a piece of the mantle of a freshwater mussel. The cilia began immediately to move much more rapidly. This was watched for some time. Ciliary motion not affected, or at all events not arrested, after more than half an hour.

Experiment XXXIV.

December 10*th*, 1874.—A piece of the gills of a freshwater mussel placed under the microscope and a little cobra-poison added at 10.40 P.M. The cilia were extremely active.

At 10.55 still active.

11.5. Several ciliated amœboid masses are now quiet instead of rolling over and over as they did, but the cilia on their surface are still moving.

11.15. The cilia on these Infusoria have now nearly all stopped. A few are moving slowly, whilst those on the gills are but little affected.

11.55. Cilia on the gills are still quite active. Those on the ciliated bodies still moving, rather more actively than before.

1.30. Cilia on gills have become much more sharply outlined. Many are standing still, though many still move briskly.

Experiment XXXV.

To another specimen a strong solution of cobra-poison was added at 10.50.

1.30. Cilia still moving.

Experiment XXXVI.

A third specimen was laid in an almost syrupy solution of dried cobra-poison at 11.28.

At 11.40 no effect observable.

1.30. Some have stopped, but numbers are still moving quite briskly.

In this case the poison seemed not to have any action on the ciliary motion.

Experiment XXXVII.

January 6*th*, 1875.—At 3.40 some diluted cobra-poison added to *Vallisneria*. Circulation going on vigorously. About $\frac{1}{10}$ grain in three drops of water.

3.58. The movements are unchanged.

5 P.M. Movements going on as before.

Experiment XXXVIII.

Added some solution of cobra-poison at 4 P.M. to another specimen of *Vallisneria*.

4.10. No change.

4.45. Circulation goes on vigorously.

4.55. Perhaps rather less brisk in their movements.

The results of these experiments show that cobra-virus must be regarded as, to a certain extent, a poison to protoplasm, seeing that it arrested with rapidity the movements in Infusoria* (*vide* Experiments XXX., XXXI., and following). Still it cannot be regarded certainly as a very powerful one, for the cilia of the freshwater mussel continued to move for many hours in a strong solution of cobra-poison; though in other experiments the action was apparently arrested even in weaker solutions of the poison. In the case of cilia from the frog's mouth, the results were more definite, but action was not invariably destroyed. The results of the action of the poison on the amœboid movements of the blood-corpuscles are not very definite. In the case of *Vallisneria*, the circulation in the cells went on with undiminished vigour after the application of the poison for two hours.

February 25, 1875.

JOSEPH DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Communication from Mr. Robert Mallet, F.R.S., was read :—

Offices, 7 Westminster Chambers,
Victoria Street, London, S.W.,
6th February, 1875.

G. G. Stokes, Esq., Secretary, Royal Society.

Dear Sir,—The large Mercator Chart of the World, which accompanies this communication, is that upon which, in time past, I and my eldest son, Dr. J. W. Mallet, laid down the distribution over the surface of our globe of earthquake occurrence, based upon the great Earthquake Catalogue of the British Association compiled by us.

The method upon which the map was prepared, which includes both frequency and intensity, has been fully described in my Reports upon the Facts and Theory of Earthquakes, published in successive volumes of the Reports of the British Association. The map itself is more particularly referred to and described in the fourth of these Reports, published in 1856. This map has remained in my possession up to the present time—a reduced copy, very far from perfect and printed in chromo-lithograph, having alone been published by the British Association.

As this map, therefore, is unique, was the result of very great labour, and, from the system upon which it was prepared, may hereafter prove

* Is this accounted for by the existence of a rudimentary nervous system diffused throughout these two forms of life, and on which the poison could act?

an important record for reference in the future progress of seismology, I have thought it desirable that it should be presented to the Royal Society, with a view to it being preserved in the Archives of the Society; and I would beg to be informed whether the Council may think fit to accept the deposit.

I remain, dear Sir,

Truly yours,

ROBERT MALLET.

The thanks of the Society were given to Mr. Mallet for his valuable Present.

The following Paper was read:—

"On the Integration of Algebraical Functions, with Illustrations in Mechanics." By W. H. L. RUSSELL, F.R.S. Received December 17, 1874.

(Abstract.)

The profound researches of Weierstrass, of Riemann, of Clebsch, and Gordan on the higher integrals have of late attracted the attention and been the admiration of mathematicians. There is, however, this difference between these researches and the corresponding investigations in elliptic functions—in the latter we investigate the properties of the integrals themselves; in the former we investigate the properties of certain differential equations, involving these integrals, and with more than one variable. How the values of the integrals themselves are to be found from these equations is difficult to see, and at all events must be a subject of enormous complexity. Accordingly it becomes desirable to ascertain, if possible, a more simple method of evaluating the integrals themselves. This is what I have attempted in the first section of this paper. I express the values of irrational algebraic quantities by means of linear differential equations with rational coefficients, and then express their integrals by means of converging series.

In the second section I consider, to a certain extent, the inverse problem—namely, to ascertain under what circumstances linear differential equations of the second order are satisfied by irrational functions. This problem I have already considered, although in an incomplete manner, in the Proceedings of the Royal Society.

In the third section I illustrate the principles enunciated in the first section by the solution of dynamical problems. I show that the principle of *vis viva* enables us to resolve these problems to a great extent by means of hyperelliptic functions and the higher transcendents.

Altogether I venture to hope that the memoir which I have the honour to lay before the Society will be read with interest by mathematicians.

THE BAKERIAN LECTURE was delivered by Prof. W. G. ADAMS, M.A., F.R.S., "On the Forms of Equipotential Curves and Surfaces and Lines of Electric Force." The following is an Abstract:—

This paper contains an account of certain experimental verifications of the laws of electrical distribution in space and in a plane conducting sheet.

The potential at any point of an unlimited plane sheet due to a charge of electricity at any other point of the plane at distance r from it is proportional to the logarithm of the distance, and the potential due to two or more charges at different points of the plane is the sum of the potentials due to the several charges; so that when there are two points in a plane conducting sheet connected with the poles of a battery, as there are equal currents flowing at those two points, one into and the other out of the sheet, the potential at any point of the sheet is proportional to the difference of the logarithms of its distances from the two points or electrodes where the current enters and leaves the sheet.

The potential is constant for a series of points if the difference of the logarithms of the distances of each of those points from the electrodes remains constant, *i. e.* if the ratio of the distances of each of those points from the electrodes remains constant.

The curve joining this series of points is an equipotential curve.

If r and r_1 are the distances of any point in the curve from the two electrodes, and c a constant, then

$$r = cr_1.$$

Hence the equipotential curves are circles with their centres on the line joining the two electrodes; and the lines of force which cut the equipotential curves at right angles are also arcs of circles passing through the two electrodes.

The lines of force may be regarded as distinct from one another, but as filling up all the space on the conductor between the two electrodes; and the distribution would not be altered if we conceive of them as divided from one another like separate wires conducting currents side by side. By taking out any space bounded by lines of force, we shall increase the quantity flowing along the other lines of force, but shall not alter the distribution of the current among them. Hence we may cut out a disk from an unlimited sheet without altering the form of the lines of force, if the boundary of the disk be arcs of circles passing through the two electrodes; so that for a circular disk with the electrodes on the edge of it, the equipotential curves are circles having their centres on the straight line joining the electrodes.

The forms of the equipotential curves may be traced out experimentally by attaching two battery-electrodes to a disk of tinfoil, and having two similar electrodes attached to a delicate galvanometer; one of these electrodes being fixed at a point through which the equipotential curve is to be drawn, the other may be moved from point to point to trace out the successive points, so that no current may pass through the galvanometer. A comparison of the experimental results with the theory shows a complete agreement.

In a large square sheet 310 millims. in diameter, with the electrodes 126 millims. apart, the curves in the centre and near the electrodes, which are drawn by pricking fine holes through the tinfoil on a sheet of paper below, are very accurately circular, and mostly coincide with circles, until the points are so far from the centre that the form of the equipotential curves is affected by the edge of the disk. In a circular disk with the electrodes on the edge subtending 60° at the centre, the experimental curves are shown to be accurately arcs of circles, with their centres on the line joining the electrodes.

In an unlimited sheet, when there are several electrodes by which currents enter and leave the sheet, the potential at any point is

$$A \log \left(\frac{r\ r'\ r'' \ldots}{r_1 r_1' r_1'' \ldots}\right),$$

where $r, r', r'' \ldots$ are the distances to the electrodes of one kind, and r_1, r_1', r_1'' are the distances to the electrodes of the other kind. Taking the case of one positive electrode at the centre and four negative electrodes round it at the corners of a square, the curves are traced and are seen to be the same as the curves at the corner of a square sheet with a positive electrode at the corner and two negative electrodes on the edges; the curves are also the same for a square sheet with a positive electrode at the corner, and one negative electrode along the diagonal.

The equation for these equipotential curves is

$$r^4 = c r_1 r_2 r_3 r_4,$$

and is derived, in the case of the limited sheets, by considering that, to every electrode on the limited sheet, there corresponds an equal and like electrode at each of the electrical images of that electrode formed by the edges of the sheet. If we trace the curves for this arrangement of electrodes in the unlimited sheet, the edges of the limited sheet will be some of the lines of force; and so we may divide the sheet along these edges, without altering the form of the equipotential curves. Where an electrode and its images coincide in position, the index of r is equal to one more than the number of images.

When there are four electrodes, two of each kind on an unlimited sheet, an equipotential curve is given by the equation

$$rr' = c r_1 r_1'.$$

If the four points lie on a circle, and the complete quadrilateral be drawn through them, the circles which have their centres at the intersections of opposite sides of the quadrilateral, and which cut the first circle at right angles, will also cut one another at right angles. One of these circles is shown to be an equipotential curve for the four electrodes, and the other is a line of force.

Hence, if we cut the unlimited sheet along the edge of this latter circle, we shall not alter the forms of the equipotential curves; and within it we shall have one electrode of each kind, the others being their electric images, the product of the distances of an electrode and its image from the centre being equal to the square of the radius of the disk. If an electrode is at the edge of the disk, then the electrode and its image coincide, and the equation to the equipotential curve is

$$r^2 = cr_1 r_2.$$

When one pole is at the edge and the other is at the centre of a circular disk, since the electric image of the centre is at an infinite distance, the equation to the equipotential curves is

$$r^2 = cr_1.$$

This is an interesting case, as showing that the equipotential curves do not always cut the edge of the disk at right angles. The curves around the centre of the disk are nearly ellipses of small eccentricity, with one focus in the centre; but on placing one tracing electrode at a distance from the centre

$$=(3-2\sqrt{2})a,$$

between the electrodes, where a is the radius, there is great uncertainty in determining the form of the curve on the opposite side of the centre of the disk.

This is explained by the fact that the electrodes were 1 millim. in diameter, and a difference of distance of 1 millim. between the electrodes near this point corresponds to a large portion of the disk on the other side of the centre—this portion including an area of about 500 square millims. in a circle 36 millims. in radius, *i. e.* about one eighth of the whole area of the circle. On placing one of the galvanometer-electrodes at the extremity of the diameter through the battery-electrodes and tracing with the other, it is found that the equipotential curve through that point cuts the edge of the disk at an angle of 45°, and that there are two branches cutting one another at right angles.

These peculiarities are explained on tracing the curve

$$r^2 = 4ar_1$$

corresponding to this case. The extremity of the diameter is a point through which two branches of the curve pass at right angles to one another.

The forms of the equipotential surfaces and lines of force in space may be determined experimentally by taking a large vessel containing a con-

ducting liquid and placing two points, the ends of two covered wires, for battery-electrodes, at a given depth in the liquid and away from the sides and ends of the vessel, taking similar covered wires, immersed to the same depth, for galvanometer-electrodes.

For two electrodes, the equipotential surfaces will be surfaces of revolution around the straight line joining them, and so will cut any plane, drawn through this straight line or axis, everywhere at right angles.

Hence we may suppose sections of the liquid made along such planes without altering the forms of the equipotential surfaces. This shows that we may place our battery-electrodes at the side of a rectangular box containing the liquid, and with the points only just immersed below the surface of the liquid; and the equipotential surfaces will be the same as if the liquid were of unlimited extent in every direction about the electrodes.

We shall obtain the section of the equipotential surface by taking for galvanometer-electrodes two points in the surface of the liquid, keeping one fixed and tracing out points of equal potential with the other.

The potential at any point in space, due to two equal and opposite electrodes, is

$$A\left(\frac{1}{r}-\frac{1}{r_1}\right),$$

where r and r_1 are the distances of the point from the electrodes; so that for an equipotential surface

$$\frac{1}{r}-\frac{1}{r_1}=\text{constant}.$$

These surfaces are cut at right angles by the curves $\cos\theta-\cos\phi=c$, which are also the magnetic lines of force, θ and ϕ being the angles which the distances from the electrodes make with the axis. That the lines of force in a vessel of finite size should agree with the lines of force in space, the form of the boundary of the vessel in a plane through the axis should everywhere be a line of force; but the ends of a rectangular vessel coincide very closely with certain lines of force, either when the electrodes are at the ends, or when there are two electrodes within the vessel, and two supposed electrodes at their electrical images at an equal distance outside the ends of the vessel.

The equipotential surfaces are given in this case by the equation

$$\frac{1}{r}+\frac{1}{r'}-\frac{1}{r_1}-\frac{1}{r_1'}=\text{constant},$$

and the lines of force by the equation

$$\cos\theta+\cos\theta_1-\cos\phi-\cos\phi_1=c.$$

The curve for which $c=2$ coincides very closely with the ends of the box.

The equipotential surfaces were traced out in sulphate of copper and in sulphate of zinc by the following method :—

A rectangular box was taken, and the battery-electrodes attached to pieces of wood, which could be clamped at the centre of the end of the box, and could be brought to any required point in the line joining the middle points of the end of the box. The galvanometer-electrodes were attached to T pieces which rest on the ends and side of the box, and the position of the electrodes was read off by a millimetre-scale placed on the ends and sides of the box.

In the sulphate-of-copper experiments, covered wire with the end exposed was immersed to half the depth of the liquid; in the experiments with sulphate of zinc, the zinc electrodes were just immersed below the surface of the liquid. The close coincidence between the experimental curves traced out and the theoretical curves and surfaces in space is shown by a comparison of the numbers given in the paper for several of the curves which have been traced out; it also shows that, by reversing currents alternately, it is easy to keep the polarization very small, and of constant amount, on the galvanometer-electrodes.

When the electrodes are parallel lines extending throughout the depth of the liquid, the equipotential surfaces are cylindrical, and their sections are given by the equation

$$\log(rr'\ldots)-(\log r_1 r_1'\ldots)=\log c,$$

where there are several positive and several negative electrodes, $r.r'\ldots$ &c. being measured from the points where the electrodes cut the plane of the section.

Hence the forms of these equipotential curves are the same as in a plane sheet; so that the forms traced out in tinfoil will be the same as the corresponding forms in space for line electrodes. These forms may be traced out in sulphate of copper with copper electrodes, or in sulphate of zinc with amalgamated zinc electrodes; and for these experiments, with cylindrical and other vessels, the polar coordinates may be measured directly. One of the battery-electrodes is made the origin of coordinates, and a lath, or brass wire, resting on the edges of the vessel has a slot along it, the origin being at one end or at some point of the slot. In the slot is a sliding piece of wood or ivory which carries one of the galvanometer-electrodes, and the lath is capable of turning about the battery-electrode on which it is placed. Around this electrode is a graduated circle for measuring the angles about the origin, and, on the sides of the slot, is a millimetre-scale for measuring the distances from the origin.

The other galvanometer-electrode may be fixed in a manner which is most suitable in each case.

The results of these investigations show how closely the experimental determination of equipotential surfaces and lines of force agrees with the theory of electrical distribution in space.

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"On the Physiological Action of the Chinoline and Pyridine Bases." By John G. M'Kendrick and James Dewar, Edinburgh. Communicated by J. Burdon Sanderson, M.D., F.R.S. Received June 11, 1874*.

It is well known that when either quinine, cinchonine, or strychnine is distilled with caustic potash, each of these substances yield two homologous series of bases, named the pyridine and chinoline series. It has also been shown by Anderson and Greville Williams that bases isomeric with these are obtained by the destructive distillation of coal or from Dippel's oil. Greville Williams has also pointed out that chinoline obtained from coal-tar differs in some respects from that got from cinchonine. It would be a subject of physiological interest to ascertain (1) the physiological action of the various members of the series; (2) whether there was any difference in this respect between the members of the series obtained from cinchonine and those got from tar; and (3) whether, and if so, how, both as regards extent and character, the physiological action of these bases differed from that of the original alkaloidal bodies. We have for some time been engaged in this research; and the results we now beg leave to lay before the Society.

I. Method of the Research.

The bases in both series are difficult to separate from each other; but this has been done, as far as possible, by repeated fractional distillation. The substance first examined was chinoline obtained from cinchonine. It was employed both as sulphate and hydrochlorate, dissolved in water, and introduced by subcutaneous injection into the animal. The strength of the solution employed in this and in all other instances was one part of the base to twenty parts of water. Its physiological action was tested on frogs,

* Read June 18, 1874. See 'Proceedings,' vol. xxii. p. 432.

mice, rabbits, guineapigs, cats, dogs, and man; but as the effects were found to be similar in all of these instances, the majority of the observations were made on rabbits. After having noted the effects of chinoline, we next studied, by the same method, the action of hydrochlorates of the bases distilling off at higher temperatures, including such bases as lepidine, dispoline, tetrahiroline, &c. We then examined the pyridine series, beginning with pyridine itself, and passing upwards to bases obtained at still higher boiling-points, such as picoline, lutidine, &c. Lastly, the investigation was directed to the action of condensed bases, such as dipyridine, parapicoline, &c.; and the effects of these substances were compared with those produced by the members of the chinoline series and among themselves. So far as we could observe, there was no difference as regards physiological action between bases obtained from cinchonine and others got from tar.

II. Physiological Effects of Hydrochlorate of Chinoline (C_9H_7NHCl).

The administration, by subcutaneous injection, of $1\frac{1}{4}$ grain for every 1 pound of weight into a healthy rabbit produced the following effects:—In four or five minutes the animal appeared to become drowsy, was unwilling to move; but when pushed, locomotion was not affected. Both the pulsations of the heart and the respiratory movements were slightly increased in frequency at this stage. The drowsiness increased, and in a few minutes more the animal sank on its abdomen and remained motionless, with the eyes widely opened. It was now gently turned over on its back or side, and it remained in that unnatural position. Still later, there was complete anæsthesia. At no period was there any hyperæsthesia. Reflex functions were also in abeyance so far that they could not be excited by pinching or pricking, but irritation by a Faradic current caused feeble movements. The animal appeared to be unconscious of loud sounds. The pupil was normal as regards size, and it contracted readily when exposed to a strong light. The reflex movements of the eyelid were not lost until the animal was in a state of deep stupor from an overdose. The respirations were now much fewer in number, and of less depth than normal. The heart still acted vigorously, but the pulsations were decreased in number by about one sixth. After remaining motionless in that condition for a period of three or perhaps four hours, the rabbit slowly recovered, raised its head from the table, began to move about, and frequently ate food placed before it. It recovered completely from the dose above indicated, without any bad symptom supervening. A dose of 2 or $2\frac{1}{2}$ grains per pound weight was usually lethal. If, at the end of three hours, the animal showed no indications towards recovery, it apparently sank in a state of profound insensibility, the heart-pulsations became feebler, and the respirations more and more shallow, until they were barely perceptible. Death ensued

without convulsions. The temperature of the body fell 6° to 8° below the normal. An examination of the body, made immediately after death, showed the following appearances:—(1) The vessels on the surface of the brain were somewhat congested; the substance of the brain itself did not exhibit any increase of vascularity; (2) the lungs were congested, more especially along the borders; (3) the heart was in a state of diastole and full of dark-coloured blood; (4) the veins in the mesentery and the larger vessels on the intestine were much congested; (5) the liver showed numerous minute ring-like congestions, indicating congestion of the portal system; (6) the kidneys and other abdominal and pelvic viscera were normal in appearance; and (7) the urine in the bladder contained no albumen or sugar.

From these symptoms and *post mortem* appearances, and from special methods of experiment, we draw the following conclusions regarding the action of hydrochlorate of chinoline.

1. *Action on the Nervous System.*—The action is chiefly, if not altogether, on the nerve-centres, and not on the nerves or on their peripheral terminations. When the sciatic nerve is irritated by very feeble Faradic currents, it manifests no diminution of sensibility, and the muscles supplied by the nerve contract with apparently their normal energy. The nerves of a frog killed by hydrochlorate of chinoline show all the properties of nerves obtained from a non-poisoned animal. The sympathetic system of nerves is not usually affected to any appreciable extent, as evidenced by the normal condition of the pupil, the absence of dilatation of the vessels in the ear (consequent on paralysis of the vaso-motor system), and by the fact that, in an animal deeply under the influence of chinoline, the phenomena following section and irritation of the sympathetic in the neck of a rabbit take place to the same extent and in the same order as in a non-poisoned animal. In several instances we have observed dilatation of the vessels of the ear and slight contraction of the pupil; but these, from their rare occurrence, appear to be exceptional. No change was observed in the action of the pneumogastric nerve, so far as could be ascertained by the effect on the heart produced by Faradic irritation of that nerve. We have observed no symptoms leading us to suspect irritation, or paralysis, of the centres in the cerebellum, or in the ganglia at the base of the brain; but in lethal doses the respiratory centres in the *medulla oblongata* become gradually affected, as shown by the diminution of the respiratory movements both in force and frequency. The motor columns of the spinal cord do not lose their power of conduction from the encephalon to various parts of the body, because, on irritating with a weak Faradic current the end of the cord in a rabbit decapitated while deeply under the influence of the substance, powerful convulsive movements ensued. The reflex activity of the cord is much weakened, inasmuch as it cannot be excited by pinching or pricking; but it is not entirely lost, because it may be excited by Faradic stimulation; and it has

been observed in several experiments that strychnine, subcutaneously injected into a rabbit prostrate with hydrochlorate of chinoline, is followed by its usual physiological effects. It appears, therefore, that the substance acts chiefly on the sensory and motor centres in the cerebral hemispheres, weakening or removing all consciousness of external impressions and also all voluntary acts.

2. *Action on the Respiratory and Circulatory Systems.*—In the first instance the action of the heart and the respiratory movements are increased, but afterwards they are much diminished, and death appears to be the result of these processes becoming weaker and weaker, until they cease altogether. The increased action observed, at first, is probably due to the excitement of the animal consequent on the injection of fluid beneath the skin. So soon as the substance acts through the blood on the nerve-centres, the action of both systems is weakened. We regard this weakening as due to an action on an encephalic centre, for the two following reasons:—first, because irritation of the sympathetic and pneumogastric nerves in the neck of a rabbit, completely under the influence of hydrochlorate of chinoline, produces acceleration and retardation of the heart's action respectively, as occurs in a healthy animal; and secondly, when the heart of a frog was treated, according to Coats's method, with serum containing 3 per cent. of chinoline, no effect was observed. These experiments seem to indicate clearly that the substance acts on the encephalic centres, and through them on the heart and respiratory organs. The action of the heart finally ceases, probably by its textures being supplied with only venous blood.

3. *Action in lowering the Temperature of the Body.*—It was found, in three instances in which minute differences of temperature were observed at intervals of one minute, during a period of one hour before and one hour after the subcutaneous injection of hydrochlorate of chinoline, that the substance produced a gradual and uniform fall of temperature to the extent of from six to eight degrees below the normal. In all of these instances the animal recovered from the effects, and, during recovery, the temperature slowly rose to its normal limit. This action we regard as of considerable importance. It is probably to be explained by interference with nutritional changes between the blood and the tissues, and also by the diminution, both in frequency and depth, of the respiratory movements.

III. Action of Hydrochlorates of the Higher Bases of the Chinoline Series.

1. *Bases obtained by distillation between* 200° *and* 280° C. *Lepidine &c.*, $C_{10}H_9N$.

These bases produced the same general action as chinoline, with the exceptions (1) that the dose required to produce a state of complete stupor was somewhat smaller than in the case of chinoline, and (2) that,

after a state of stupor had been produced, the animal was less likely to recover, while it was observed that, frequently, before death, there were jactations of one or other of the limbs and convulsive twitchings about the mouth.

2. *Bases obtained by distillation between* 280° *and* 300° C. *Dispoline &c.*, $C_{11} H_{11} N$.

It was now observed that the symptoms following subcutaneous injection were considerably different from those of chinoline. One grain for each pound weight of the rabbit produced, in about five minutes, apparent uneasiness, side to side movements of the head, with a tendency occasionally to move backwards. This condition continued for three or four minutes, when the animal lay flat on its abdomen with its legs outspread. It was not in a state of complete unconsciousness. There was no anæsthesia. In several instances there were compulsive twitchings of the limbs, grinding of the teeth, and a slight tendency to opisthotonos. The lethal dose was smaller than in the case of chinoline. The effects were a longer time in appearing, and they had more of a spinal than of a cerebral character.

3. *Bases obtained by distillation above* 300° C. *Tetrahiroline &c.*, $C_{12} H_{13} N$.

These were found to be still more active. A dose of $\frac{3}{4}$ of a grain per pound weight produced, in eight or ten minutes after it had been subcutaneously injected, violent convulsions, and was almost invariably fatal. During the first five minutes after the introduction of the poison, no marked symptoms were noticed. At the end of this time, the animal became uneasy, ran forwards in an excited manner, and then fell over on its side. The convulsions which ensued were similar to those produced by the action of large doses of quinine or cinchonine. They did not resemble the tetanic spasms produced by strychnia, as they were not excited by peripheral irritations; but they had an epileptiform character, consisting of irregular jactations of the limbs, crunching of the teeth, movements of the eyeballs, pawing movements of the fore limbs, &c. The animal seemed to be semiconscious throughout. It was still susceptible to pain.

A consideration of the effects just described indicates that, as we ascend from the lower to the higher members of the chinoline series of bases, the physiological action becomes modified as follows:—

1. The action on the sensory centres of the encephalon becomes less marked, until in the highest group there is no unconsciousness, but only slight stupor.

2. The actions of the motor centres of the encephalon and spinal cord are not affected in the lowest group, but become gradually more and

more involved as we proceed upwards, until, in the highest group, we have substances producing powerful convulsions.

3. The lethal dose is smaller for the higher than for the lower members of the series.

IV. Action of Hydrochlorates of the Bases in the Pyridine Series.

The physiological action of the bases of the pyridine series was next examined in the following order :—

1. *Pyridine*, $C_5 H_5 N$.

The hydrochlorate of this base produced no effects, even in doses of 6 grains per pound weight, other than slight excitement and acceleration of the pulse and of the respiratory movements. The animal, judging from its gait and demeanour, appeared to be in a state analogous to intoxication. It recovered without any bad effects.

2. *Picoline*, $C_6 H_7 N$.

The substance was employed both in the form of the base dissolved in water and as a hydrochlorate. The salt was found to be more active physiologically than the base, but the kind of action was the same. The general effect was to produce, with a dose of 3 grains per pound weight, in the first place, general excitement and a full bounding pulse. This state was followed by a drowsy condition, which did not pass, with even a dose of 6 grains per pound weight, into complete stupor. The rabbit could always be readily aroused. While in the drowsy condition, the pulse fell in frequency and volume, and the respirations became feebler*.

3. *Lutidine*, $C_7 H_9 N$.

The effects were similar to those produced by picoline, only more marked. A dose of 3 grains per pound weight produced deep stupor, from which the animal could not be aroused. It remained in this condition for a period of from two to three hours. The pulsations of the heart were much reduced in volume, but only slightly in frequency; but it was clearly observable that the respirations were much less deep than in the natural condition, and they were reduced in frequency by about one third. In a case of death from a lethal dose of 4 grains per pound, there was venous congestion in all parts of the body, but the heart was still feebly pulsating. It was observed that the blood had a peculiar dark chocolate-brown appearance. Examined with the spectroscope, it showed the two bands of oxyhæmoglobin.

* The results we have obtained differ considerably from those described by H. Vohl and H. Eulenberg in their paper on the "Physiological Action of Tobacco when used as a Narcotic, with especial reference to the Constituents of Tobacco-Smoke," Archiv Pharm. [2] cxlvii. 130–166.

4. *Collidine*, $C_8 H_{11} N$.

Collidine was still more active in its effects. With a dose of $1\frac{1}{2}$ grain per pound weight, the animal rapidly sank into a state of profound stupor, from which it could not be aroused. Anæsthesia was complete. The pulsations of the heart and the respirations became more and more feeble, until death ensued in about 20 minutes after the dose, apparently in consequence of failure of respiration. There were no twitchings or convulsions. The subcutaneous injection into a rabbit of 1-80th of a grain of strychnine was followed by the usual physiological effects of that substance.

5. *The Higher Pyridine Bases obtained by distillation above* 200° C., *such as Parvoline*, $C_9 H_{13} N$, &c.

These were found to be still more active; but the effects were of the same nature as those just described. The lethal dose was found to be about $\frac{3}{4}$ of a grain per pound weight. In two or three minutes the animal sank on its abdomen; when pushed could move with difficulty; respirations were rapid and irregular. It then lay on its side, and in four or five minutes died, apparently in an asphyxiated condition. There were no convulsive spasms or twitchings. This substance was lethal in much smaller doses than the lower bases of the chinoline series.

The pyridine series of compounds thus showed a gradual increase in activity of physiological action. The lowest of the series produced merely excitement from irritation of the encephalic nervous centres, while the highest produced paralysis of these nervous centres. There was no irritation of the spinal cord causing increased reflex activity. Death ensued from gradual failure of the respiratory movements leading to asphyxia. The action of the higher pyridines was thus somewhat analogous to the lowest of the chinoline series, with this exception, that the pyridine compounds tended to cause death by asphyxia. It is to be noted also that the higher bases of the pyridine series were lethal in somewhat less than one half of the dose required to destroy life by the lower members of the chinoline series.

V. Action of Hydrochlorates of the condensed Bases of the Pyridine Series.

Considering the close analogy in chemical composition between the polymeric bases of pyridine and certain natural bases, such as nicotine, it became of importance to examine the physiological action of these bases, which were prepared, according to Anderson's method, by the action of sodium on pyridine, picoline, &c. The following were the effects observed after the subcutaneous injection of 1 grain per pound weight into a rabbit:—The animal remained quiet for a period varying from four to eight minutes, when it suddenly appeared uneasy, ran forwards as on tiptoe, with the back arched, and, falling on its side, became

violently convulsed. The convulsions continued, almost without intermission, for three or four minutes, when death ensued. So far as could be observed, consciousness was not lost until immediately before death. The character of the convulsions resembled that of those produced by cinchonine or quinine, except that the tendency to backward movements, with the fore legs extended, was not so marked; they also resembled those produced by salts of the higher members of the chinoline series, but they were more severe than in the latter. The hydrochlorates of two condensed bases of this kind were employed—the first made from pyridine, and the other from picoline. The formulæ for these are:—hydrochlorate of dipyridine, $C_{10} H_{10} N_2$ 2HCl; and hydrochlorate of dipicoline, or parapicoline, $C_{12} H_{14} N_2$ 2HCl. The latter was found to be the more active of the two, but the actions were identical in character.

VI. General Conclusions.

1. There is a marked gradation in the extent of physiological action of the members of the pyridine series of bases, but it remains of the same kind. The lethal dose, however, becomes reduced as we rise from the lower to the higher.

2. The higher members of the pyridine series resemble, in physiological action, the lower members of the chinoline series, except (1) that the former are more liable to cause death by asphyxia, and (2) that the lethal dose of the pyridines is less than one half that of the chinolines.

3. In proceeding from the lower to the higher members of the chinoline series, the physiological action changes in character, inasmuch as the lower members appear to act chiefly on the sensory centres of the encephalon and the reflex centres of the spinal cord, destroying the power of voluntary or reflex movement; while the higher act less on these centres, and chiefly on the motor centres, first as irritants, causing violent convulsions, and afterwards producing complete paralysis. At the same time, while the reflex activity of the centres in the spinal cord appears to be so far inactive as not to be excited by pinching or pricking, it may be readily roused to action by strychnine.

4. On comparing the action of such bases as $C_9 H_7 N$ (chinoline) with $C_9 H_{13} N$ (parvoline), or $C_8 H_{11} N$ (collidine) with $C_8 H_{15} N$ (conia from hemlock), or $C_{10} H_{10} N_2$ (dipyridine) with $C_{10} H_{14} N_2$ (nicotine from tobacco), it is to be observed that, apart from differences in chemical structure, the physiological activity of the substance is greater in those bases containing the larger amount of hydrogen.

5. Those artificial bases which approximately approach the percentage composition of natural bases are much weaker physiologically, so far as can be estimated by amount of dose, than the natural bases; but the kind of action is the same in both cases.

6. When the bases of the pyridine series are doubled by condensation, producing dipyridine, parapicoline, &c., they not only become more

active physiologically, but the action differs in kind from that of the simple bases, and resembles the action of natural bases or alkaloids having an approximately similar chemical composition.

7. All the substances examined in this research are remarkable for not possessing any specific paralytic action on the heart likely to cause syncope; but they destroy life, in lethal doses, either by exhaustive convulsions or by gradual paralysis of the centres of respiration, thus causing asphyxia.

8. There is no immediate action on the sympathetic system of nerves, although there is probably a secondary action, because, after large doses, the vaso-motor centre, in common with other centres, becomes involved.

9. There is no appreciable difference between the physiological action of the bases obtained from cinchona and those derived from tar.

The physiological action of the substitution derivatives of these substances will be related in a further communication.

March 4, 1875.

JOSEPH DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The Right Hon. Sir Stafford H. Northcote was admitted into the Society.

In pursuance of the Statutes, the names of the Candidates for election into the Society were read as follows:—

William de Wiveleslie Abney, Capt. R.E.
William Archer, M.R.I.A.
Edward Middleton Barry, R.A.
James Risdon Bennett, M.D.
Dietrich Brandis, Ph.D.
Charles Orde Browne, Capt. R.A.
George Buchanan, M.D.
Walter Lawry Buller, Sc.D.
James Caird, C.B.
John Casey, LL.D.
William Chimmo, Capt. R.N.
Latimer Clark, F.R.A.S.
Cuthbert Collingwood, M.A.
George Critchett, F.R.C.S.
Herbert Davies, M.D.
August Dupré, Ph.D.
Joseph Fayrer, M.D.
Augustus H. Lane Fox, Colonel.
Francis Stephen Bennet François de Chaumont, M.D.
Alfred Henry Garrod, B.A.
James Geikie, F.R.S.E.
James Whitbread Lee Glaisher, M.A.
Thomas Minchin Goodeve, M.A.
Charles Alexander Gordon, M.D., C.B.
Robert Baldwin Hayward, M.A.
John Baboneau Nickterlien Hennessey, F.R.A.S.
John Hughlings Jackson, M.D.
Emanuel Klein, M.D.
E. Ray Lankester, M.A.

Squire Thornton Stratford Lecky, Lieut. R.N.R.
Robert M'Lachlan, F.L.S.
Richard Henry Major.
John William Mallet, Ph.D.
George Strong Nares, Capt. R.N.
Robert Stirling Newall, F.R.A.S.
Oliver Pemberton.
David Simpson Price, Ph.D.
William Roberts, M.D.
William Chandler Roberts, F.C.S.
William Rutherford, M.D.
Henry Young Darracott Scott, Major-General, C.B.
Harry Govier Seeley.
Joseph Sidebotham.
John Spiller, F.C.S.
Robert Swinhoe.
George James Symons, V.P.M.S.
Sir Henry Thompson, F.R.C.S.
Thomas Edward Thorpe, Ph.D.
Charles Todd (Obs., Adelaide).
Edwin T. Truman, M.R.C.S.
Wildman Orange Whitehouse, C.E.
Thomas Alexander Wise, M.D.
Archibald Henry Plantagenet Stuart Wortley.
Sir Matthew Digby Wyatt, Knt.

The following Papers were read :—

I. "On the Tides of the Arctic Seas.—Part VI. Tides of Port Kennedy, in Bellot Strait, in July 1859." By the Rev. SAMUEL HAUGHTON, M.D. Dublin, D.C.L. Oxon., F.R.S., Fellow of Trinity College, Dublin. Received January 20, 1875.

(Abstract.)

These observations were made on board the yacht 'Fox,' under the command of Sir Leopold M'Clintock, during his successful search for the remains of the Franklin Expedition.

The heights of the tide were observed every hour during 23 days, and the results obtained were extremely interesting.

The tides of Port Kennedy are remarkable for two points :—

1. The magnitude of the diurnal tide.
2. The solar diurnal tide is greater than the lunar diurnal tide.

The following tidal constants have been successfully determined :—

I. DIURNAL TIDE.

Solar Diurnal Tide.

1. Age	Unknown.
2. True Solitidal Interval	$5^h\ 12^m\ 7\frac{1}{2}^s$.
3. Coefficient	23·4 inches.

Lunar Diurnal Tide.

1. Age	$1^d\ 4^h\ 14\frac{1}{2}^m$ (time). $4\ \ 6\ \ 20\frac{1}{2}$ (height).
2. True Lunitidal Interval	$0^h\ 33^m\ 50^s$.
3. Coefficient	18·4 inches (time). 23·37 ,, (height).

II. Semidiurnal Tide.

Lunar Semidiurnal Tide.

True Lunitidal Interval............ $23^h\ 48^m\ 1^s$.

Uncorrected ratio of Solar and Lunar Coefficients ...... $\frac{S''}{M''} = \begin{cases} 0{\cdot}412 \text{ inch (height).} \\ 0{\cdot}549 \quad \text{,, \quad (time).} \end{cases}$

II. "Note on the Value of a certain Definite Integral." By I. Todhunter, M.A., F.R.S., Honorary Fellow of St. John's College, Cambridge. Received February 13, 1875.

Let $P_m(x)$ denote Legendre's coefficient of the order m, and $P_n(x)$ that of the order n: it is required to find the value of $\int_0^1 P_m(x)\,P_n(x)dx$. We need not consider the case in which $m=n$; for it is an established result that the value of the integral taken between the limits -1 and 1 is then equal to $\frac{2}{2n+1}$, and the value between the limits 0 and 1 will be half of this. We suppose now that m and n are different.

It is known that

$$P_m(x) = \frac{1\,.\,3\,.\,5\,.\,.\,(2m-1)}{\lfloor m}\left\{x^m - \frac{m(m-1)}{2(2m-1)}x^{m-2} + \ldots\right\}, \quad . \quad . \quad (1)$$

and also that

$$\frac{d}{dx}\left\{(1-x^2)\frac{dP_m(x)}{dx}\right\} + m(m+1)\,P_m(x) = 0. \quad . \quad . \quad . \quad . \quad (2)$$

Similar expressions, of course, hold for $P_n(x)$.

Now, by a well-known process of integration by parts, we deduce

$$\{m(m+1)-n(n+1)\}\int P_m(x)\,P_n(x)dx$$

$$=\left\{P_m(x)\frac{dP_n(x)}{dx} - P_n(x)\frac{dP_m(x)}{dx}\right\}(1-x^2);$$

this formula can also be immediately verified by differentiation and the use of (2).

Thus

$$\{m(m+1)-n(n+1)\}\int_0^1 P_m(x)\,P_n(x)dx$$

$$=\left\{P_n(x)\frac{dP_m(x)}{dx} - P_m(x)\frac{dP_n(x)}{dx}\right\}_{x=0}.$$

If m and n are both even or both odd, the right-hand member of the last formula will vanish by (1); thus the only case we have to consider

is that in which one of these quantities is even and the other odd. Put, then, $2m$ for m, and $2n-1$ for n; thus we have

$$\{2m(2m+1)-(2n-1)2n\}\int_0^1 P_{2m}(x)\,P_{2n-1}(x)dx$$

$$= \left\{P_{2n-1}(x)\frac{dP_{2m}(x)}{dx} - P_{2m}(x)\frac{dP_{2n-1}(x)}{dx}\right\}_{x=0}$$

$$= -\left\{P_{2m}(x)\frac{dP_{2n-1}(x)}{dx}\right\}_{x=0}.$$

Then by (1) we obtain finally

$$\{2m(2m+1)-(2n-1)2n\}\int_0^1 P_{2m}(x)\,P_{2n-1}(x)dx$$

$$=(-1)^{m+n}\frac{1.3.5..(2m-1)}{2.4..2m}\cdot\frac{1.3...(2n-1)}{2.4..(2n-2)}.$$

This formula will be found to include the results which are given in the Philosophical Transactions for 1870, pages 579–587.

February 11, 1875.

III. "On the Determination, at Sea, of the Specific Gravity of Sea-water." By J. Y. Buchanan, Chemist on board H.M.S. 'Challenger.' Communicated by Prof. Wyville Thomson, F.R.S. Received January 22, 1875.

(Abstract.)

In the investigation of the physical condition of the ocean the accurate determination of the specific gravity of the water holds a first place. The tolerably numerous observations which have been made in this direction, in a more or less connected manner, are sufficient to prove that the density of the water varies, not only with the latitude and longitude, but also with the distance from the surface of the source from which it is taken. This difference of density depends partly on an actual difference in saltness, and partly on a difference in temperature of the water. The amount of effect due to each of these causes can be precisely stated when we know the effect of one of them, the sum of the effects of the two being given by our observations. Hence, to determine the saltness, we eliminate the effect of temperature by reducing the results to their value at one common temperature. It is also necessary that the means of obtaining the water should be of a reliable character. In estimating, therefore, the trustworthiness of the results, we must consider, first, the means used for collecting the water; second, those used for determining the relation between its weight and volume; third, the determination of

its temperature; and fourth, the reduction of the results to their value at a common temperature. These divisions of the subject are treated in their order.

The samples of water are collected either in an ordinary canvas bucket or in one of two kinds of metal "water-bottle," according as it is to be taken from the surface or from depths below it. The use of the ordinary hand-bucket needs no explanation. When water is to be obtained from the bottom, the "slip" water-bottle is used. This instrument is a Swedish invention, improved by Dr. Meyer, of Kiel, who without doubt has described it, and by Messrs. Milne, of Edinburgh, who furnished those used on board the 'Challenger.'

Water from intermediate depths is obtained in a much lighter instrument, which, with a drawing and the method of using it, is fully described in the paper of which this is an abstract. In principle it consists of a metal cylinder furnished with stopcocks at both ends. The levers by which these stopcocks are turned are connected by a straight rod, so that they are simultaneously either open or shut, or at least at the same phase of being open or shut. When water is to be collected by its means, the stopcocks are opened and the instrument sunk to the required depth, having been previously securely fastened to a sounding-line. The operation of sinking must be carried on without a check, owing to the peculiarity of the closing-apparatus. When the required depth has been reached, the line is checked, hauled in a few fathoms, let go again, and finally brought to the surface by means of the donkey-engine. The rod connecting the stopcocks is furnished with a metal plate, which, during the descent, is retained in a vertical position by the passage of the water on both sides of it. When, however, the direction of motion is reversed, the plate falls down into a horizontal position, when, by its passage through the water, it exercises such a downward pressure on the rod that the stopcocks are closed. Arrived at the surface, it contains the water which it had enclosed at the depth in question. A small safety-valve allows of the escape of the surplus water, which, owing to the greater density of the water below the surface, it has enclosed in excess of what it can hold at atmospheric temperature and pressure. The apertures of the stopcocks being necessarily smaller than the diameter of the cylinder, the efficiency of the instrument in really changing the water as it descends was tested before leaving England in a freshwater lake, the water with which it was filled at the surface containing some yellow prussiate of potash. It was found that the water fetched, under these circumstances, from depths over $1\frac{1}{2}$ fathom was unacted upon by solution of perchloride of iron. The rate, therefore, of change of water is satisfactory, as we can be certain of obtaining an average sample of, at the most, the last 2 fathoms passed through by the instrument.

The variations in the specific gravity of the water which forms the ocean are, comparatively speaking, so slight, that an instrument of con-

siderable delicacy is necessary for recording them. As far as I have hitherto been able to observe, they lie between the extremes 1·02780 and 1·02400; the results, therefore, to be of any value, must be correct, at least to one in the fourth decimal place. In mentioning these extremes, it must be observed that they refer to *ocean*-waters, and not to the mixtures of fresh and salt water to be found in bays and estuaries, where waters of all degrees of saltness may be found. The instrument selected was the hydrometer; and the purpose which it was to serve being of so very special a nature, I preferred to have a special instrument made for it, to making use of the hydrometer ordinarily supplied by the instrument-maker. It has a large body and fine stem, the relation in size of the one to the other, and the absolute size of both of them, having been determined beforehand by calculation, so as to obtain the requisite delicacy. It is evident that, for a hydrometer of given size, in the measure that its delicacy is increased its range is diminished. In determining the specific gravities of sea-waters both great delicacy and considerable range are required; the latter is secured, without detriment to the former, by the application of the principle of Nicholson's hydrometer. In the paper of which this is an abstract, the construction, calibration, and method of observing the instrument are minutely described and illustrated by a drawing. The description of the instrument is briefly as follows:—The stem, which carries a millimetre-scale 10 centimetres long, has an outside diameter of about 3 millimetres, the external volume of the divided portion being accurately 0·8607 cubic centimetre; the mean volume of the body is 160·15 cubic centimetres, and the weight of the glass instrument is 160·0405 grammes. With this volume and weight it floats in distilled water of 16° C. at about the lowest division (100) of the scale. In order to make it serviceable for heavier waters, a small brass table is made to rest on the top of the stem, of such a weight that it depresses the instrument in distilled water of 16° C. to about the topmost division (0) of the scale. By means of a series of six weights, multiples by 1, 2, 3, 4, 5, and 6 of the weight of the table, specific gravities between 1 and 1·03400 can be observed. It is not necessary that these weights should be accurate multiples of the weight of the table; it is sufficient if they approach it within a centigramme and their actual weight be known with accuracy. The weights of the table and weights in actual use are:—

Weight of table	0·8360	gramme.
Weight of weight No. I.	0·8560	,,
,, ,, II.	1·6010	,,
,, ,, III.	2·4225	grammes.
,, ,, IV.	3·2145	,,
,, ,, V.	4·0710	,,
,, ,, VI.	4·8245	,,

For oceanic waters the hydrometer is always used with the table and either No. IV. or No. V. weight.

When the mechanical part of the construction of the instrument was finished, with the exception of the closing of the top of the stem (which, instead, was widened into a funnel-shape large enough to receive the ordinary decigramme weights), the calibration of the stem was effected by loading the stem with successive weights, and observing the consequent depressions in distilled water of known temperature. This done, the top was sealed up and the instrument carefully weighed. The expansion of the body with temperature was determined in a similar manner by reading the instrument in distilled water of various temperatures. The coefficient of expansion of the glass was then found to be 0·000029 per degree Centigrade.

For using this instrument at sea about 900 cubic centimetres of water are taken, and the containing cylinder placed on a swinging table, in a position as near the centre of the ship as possible. The observation with the hydrometer, loaded with the necessary table and weight, is then effected in the ordinary way, the accuracy of the readings being but little affected by rolling; pitching, however, is found to have a distinctly disturbing effect, and, when it is in any way violent, it is advisable to store the specimen of water till the weather improves. The precautions to be observed in making these observations at sea must be sought for in the paper.

The temperature of the water, at the time of observation, is determined by one of Geissler's "normal" or standard thermometers, graduated into tenths of a degree Centigrade; and it is essential for the accuracy of the results that the water, during the observation of the hydrometer, should be sensibly at the same temperature as the atmosphere, otherwise the changing temperature of the water makes the readings of both the hydrometer and the thermometer uncertain. At low temperatures (below 10° or 12° C.) a tenth of a degree makes no sensible difference in the resulting specific gravity; but at the high temperatures always found at the surface of tropical seas, rising sometimes to 30° C., the same difference of temperature may make a difference of 3 to 4 in the fifth decimal place of the resulting specific gravity.

Having obtained the specific gravity of the water in question at a temperature which depends upon that of the air at the time, it is necessary, in order that the results may be comparable, to reduce them to their value at one common temperature. For this purpose a knowledge of the law of expansion of sea-water with temperature is necessary. This had been determined with sufficient accuracy for low temperatures by Despretz and others; but as the temperatures at which specific-gravity observations are usually made are comparatively high, their results were of but little use, directed as they were chiefly to the determination of the freezing- and maximum-density points. When the late Captain Maury was developing

his theory of oceanic circulation, owing to difference of density of the water in its different parts, he found the want of information on this important subject. At his request, the late Professor Hubbard, of the National Observatory, U.S., instituted a series of experiments, from which he was enabled to lay down a curve of the volumes of sea-water at all temperatures, from considerably below the freezing-point to much above what obtains even in tropical atmospheres. The results are published in Maury's 'Sailing Directions,' 1858, vol. i. p. 237, and have evidently been carried out with great care. The composition of different oceanic waters varies, even in extreme cases, within such close limits, that the law of thermal expansion is sensibly the same for all of them; of this Hubbard's experiments afford satisfactory proof. In the Table which gives the results of all his experiments he takes the volume of water at 60° F. as his unit. In order to avoid much useless calculation, I have been in the habit of reducing my results to the same temperature (15°·56 C.), while, for a like reason, I have retained the specific gravity of distilled water at 4° C. as the unit. The choice of a common temperature to which the results should be reduced, and of a unit of specific gravities, is a purely conventional matter; and in choosing the above-mentioned ones, in the first instance I was moved solely by a desire to save calculation. For every water, however, there is one temperature to which it would be *natural* to reduce its specific gravity—namely, the temperature which the water had when in its place in the ocean; and in this sense all my results during the cruise have been reduced. Hubbard's Table of the change of volume of a mass of sea-water with change of temperature enables us very easily to reduce any observed specific gravity from the temperature of observation to any other temperature, say 15°·56 C. In the paper it is transcribed from the 'Sailing Directions.' In the following Table the volumes for every Centigrade degree from −1° C. to +30° C. are given:—

Table I.

Temp. ° C.	Volume.	Temp. ° C.	Volume.	Temp. ° C.	Volume.	Temp. ° C.	Volume.
−1	0·99792	+7	0·99853	+15	0·99987	+23	1·00194
0	795	8	866	16	1·00010	24	224
+1	799	9	878	17	034	25	256
2	804	10	893	18	059	26	288
3	812	11	910	19	086	27	320
4	820	12	927	20	111	28	352
5	830	13	947	21	137	29	385
6	840	14	967	22	164	30	420

Let the specific gravity of a sea-water be found to be y', at a temperature t, and let v be the volume found in the above Table corresponding to $t°$. Then we shall have, for the value of the specific gravity reduced to 15°·56 C.;

$$x' = vy'.$$

Similarly, any other observed specific gravity y'', at the same temperature t, becomes at 15°·56 C.,

$$x'' = vy''.$$

In a system of rectangular coordinates, let observed specific gravities be measured along the axis of y, and reduced ones, on the same scale, along the axis of x. We have then two points (vy', y') and (vy'', y''), and the equation to the straight line passing through them is

$$y = \frac{1}{v}x.$$

This line passes through the origin and makes an angle $\tan^{-1}\frac{1}{v}$ with the axis of x. By giving to v the successive values found in the above Table for different temperatures, we can draw a system of lines all branching from the origin, and each one representing the relation between the specific gravity of different sea-waters at 15°·56 C. and at same temperature of observation t. If the values of v have been taken for every degree Centigrade within the limits of the above Table, then we have the *isothermals* for every degree Centigrade. In the extended paper an isothermal chart of this kind is given in Plate II. In it ten lines are drawn for every two degrees Fahrenheit, and the origin has been shifted from the point of zero specific gravity to a point (a, a), where $a = 1·02000$; so that the general equation to the lines is

$$y = \frac{1}{v}x + a\frac{1-v}{v}. \quad \ldots \ldots \ldots \quad (1)$$

By means of a chart of this kind the work of reducing observed specific gravities to their value at any other temperature becomes a mere mechanical operation. The observed specific gravity is laid off on the axis of y, and, through the point so found, a line is drawn parallel to the axis of x. Through the point of intersection of this line with the isothermal for the temperature of observation, a line is drawn parallel to the axis of y. This line cuts all the isothermals, and the ordinates of the points of intersection are the specific gravities of the sea-water in question at these different temperatures. The specific gravity at 15°·56 C. (60° F.) is given by the intersections—namely, where the line cuts the isothermal for 15°·56, whose equation is $y = x$, and where it cuts the axis of x; the two intersections give, of course, identical values.

In all these reductions the unit remains the same—namely, the specific gravity of distilled water at 4° C. In hitherto published observations on the specific gravity of sea-water, the unit, where mentioned at all, is frequently the specific gravity of distilled water at other temperatures, such

as 14° C., 16° C., and so forth. In order to reduce results from their value, the unit being water at 4° C., to their value the specific gravity of distilled water at any other temperature being unity, it is only necessary to divide the result by the specific gravity of distilled water at this temperature, the unit being water at 4° C. Let x' be the specific gravity of a sea-water reduced to 15°·56, and let it be required to convert it into its value when the unit is water at t°. Let c be the specific gravity of distilled water at t, that at 4° C. being unity, we have then for the specific gravity with new unit

$$y' = \frac{1}{c} x' \text{ or } x' = cy'.$$

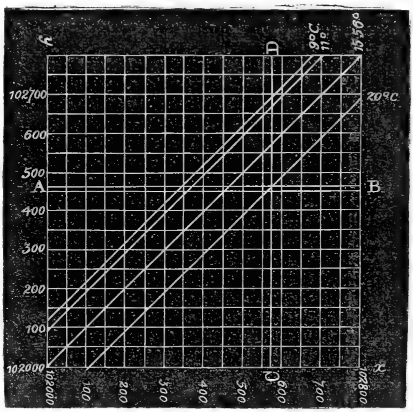
Similarly, if any other water be taken of the specific gravity x'' at 15°·56, its value at 15°·56 C., water at t° being unity, will be

$$y'' = \frac{1}{c} x'' \text{ or } x'' = cy''.$$

And, just as before, we get the general equation to a system of lines, which, when the origin is the point (a, a), is

$$y = \frac{1}{c} x + \frac{1-c}{c}. \quad \ldots\ldots\ldots \quad (2)$$

Now in Table I. the extreme values of v are 0·99792 and 1·00420, and for temperatures below 22° C. the extreme values of c are 0·99789 and 1. For any value of c, between those extremes, the line expressed by equation (2) must coincide with one represented by equation (1). By comparing, then, the values of v in Table I. with those of c in any Table of the expansion of distilled water, the lines which represent specific gravities at 15°·56, their unit being water at any temperature t, may be found which correspond to isothermals represented by equation (1). For instance, the value of c for 15°·56 is 0·99910, and in Table I. the value of v for 11° C. is also 0·99910. For these values, then, equations (1) and (2) represent identical lines. Hence, to reduce any observed specific gravity to its value at 15°·56 C., water at 15°·56 C. being unity, the same construction is made as for reducing to 11° C., the unit remaining the same; only the intersection of the line parallel to the axis of y with the isothermal to 11° C. is taken, and the ordinate of the point is the specific gravity at 15°·56 C., water at 15°·56 C. being unity.



In a precisely similar manner we find the equation to the isothermal for specific gravities reduced to any temperature t, water

at any temperature t' being unity, to be

$$y=\frac{1}{vc}x+a.\frac{1-vc}{vc}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Thus for $t=t'=11°$ C. we have $v=0·99910$, $c=0·99966$, whence $vc=0·99876$. Now in Table I. 0·99878 is the value of v for the isothermal for 9° C. Hence to reduce observed specific gravities to their values at 11° C., water at 11° C. being unity, is equivalent to reducing it to 9° C., water at 4° C. being unity, the isothermal for which is given in the chart.

To take an example, let the specific gravity of a water be observed at 20° C. and found to be 1·0247. Finding 1·0247 on the axis of y, we draw through it the line A B parallel to the axis of x. Through the intersection of A B with the isothermal for 20° C., the line C D is drawn parallel to the axis of y. The ordinates of its points of intersection with the other isothermals are the specific gravities at these temperatures, water at 4° C. being unity; they are at 15°·56 1·02575 (both by the intersection with the isothermal and with the axis of x), at 11° 1·02665, and at 9° 1·02685. When the unit is the specific gravity of distilled water at 15°·56, the specific gravity at 15°·56 is 1·02665, or the same as at 11°, water at 4° C. being unity. When the specific gravity of distilled water at 11° C. is unity, the specific gravity at 11° C. is 1·02685, or the same as at 9° C., when water at 4° is unity.

During the course of the cruise a considerable number of waters have had their specific gravities determined at different temperatures. The results when reduced to the common temperature of 15°·56 C. agree, as a rule, within 5 in the fifth decimal place, the greatest difference being 14.

When the specific gravity of the same water is determined with the same hydrometer and table, but with different weights, the mean discrepancy in the results is 5, and the maximum 10 in the fifth decimal place.

As an absolute test of the trustworthiness of the results, I landed my balance, and determined the specific gravity of the water of Hong-Kong harbour, in the usual way, by weighing it in a specific-gravity tube, and reducing it to its value at 15°·56 both by the graphical method, already described, and arithmetically, using the factors found in the two Tables of expansion of sea and distilled waters. I found its specific gravity reduced arithmetically to be 1·02391, reduced graphically 1·02394. Determined on board by means of the hydrometer and reduced graphically, it was 1·02397. The true specific gravity must therefore lie between 1·02391 and 1·02397. Taking, therefore, into account the results of the application of those different tests of accuracy, I think the trustworthiness of the method is fully established.

H.M.S. 'Challenger,' Hong Kong,
14th December, 1874.

March 11, 1875.

JOSEPH DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On Traumatic Inflammation of Connective Tissue." By G. THIN, M.D. Communicated by Professor HUXLEY, Sec. R.S. Received February 6, 1875.

(Abstract.)

The author, referring to observations recorded in his previous papers, distinguishes in the cornea primary bundles of fibrillary tissue, which are covered by elongated flat cells, layers of quadrangular flat cells (which are analogous in appearance and relative position to the layers of cells described by him as investing the secondary and tertiary bundles of tendon), and the stellate cells. To these he now adds a description of parallel chains of spindle cells, each cell having two processes, one at each end of the spindle, by which it is joined to its fellows on either side. These cells are coextensive with the cornea-substance, and are present in every interspace of the primary bundles, and, consequently, layers in different planes cross each other at an angle.

They can be occasionally seen in thin vertical sections of the fresh frog's cornea, treated in osmic acid; and from such preparations a cell with its terminal processes can be sometimes isolated. They are more easily seen in similar sections which have been 15–30 minutes in half per cent. solution of chloride of gold and then sealed up in concentrated acetic acid and examined 24–48 hours afterwards.

They have no anatomical continuity with the stellate cells.

In the fresh frog's cornea examined entire in serum, the structure, looked at through the anterior epithelium, can be seen to be broken up by clefts, the borders of which have a double contour. These clefts extend from the epithelium to a varying depth into the fibrillary tissue. They are arranged sometimes concentrically, and sometimes in waving lines which give off branches which are narrower as they approach the centre of the cornea. The double-contoured borders are not parallel to the median plane of the cornea, and can be traced only by changing the focus.

From the existence of these clefts the author infers a division of the cornea-substance into compartments equivalent to the secondary and tertiary bundles of tendon.

In inflammation the clefts are much widened, and their finer ramifications become visible. In preparations of inflamed cornea different tracts

of cornea-substance bounded by the clefts are coloured of different shades by chloride of gold, the difference affecting the fibrillary tissue, and more markedly the spindle cells.

The serous contents of the interspaces of the inflamed cornea differ in character from those of the healthy cornea, inasmuch as the former show, more abundantly, the dark granular substance which results from the reduction of the chloride of gold.

In a very early stage of inflammation (after a few hours) the distension of the narrow spaces between the primary bundles and of the wider and more yielding spaces between the lamellæ, corresponding to the larger bundles, favours the action of chloride of gold; and preparations can thus be obtained by this reagent which show that the two kinds of flat cells which cover the respective surfaces are arranged after the manner of an epithelium. The cells thus seen can be identified by their size, contour, and arrangement as those which are isolable from the healthy cornea by warm saturated solution of caustic potash, and which can be seen in preparations sealed up in aqueous humour.

A similar distension occasionally permits the demonstration of the layers covering the secondary bundles of tendon.

That the successful gold reaction, in such cases, is probably due solely to the distension of the interspaces, is inferred from the fact that, in the tendo Achillis of frogs which have died from disease and have been some hours in water after death, the author has obtained gold preparations showing not only the cells of the secondary bundles (Ranvier's cells), but also small groups of the long narrow cells which cover the primary bundles.

In the cauterized frog's cornea, examined in blood-serum after twelve hours' inflammation, portions of the primary bundles are found lying loose on the surface. These detached portions have a nearly constant length, a uniform breadth, sharply defined even borders, are sometimes puckered transversely, occasionally show a faint appearance of longitudinal fibrillation, and are sometimes cut transversely, at one or more points, by straight hyaline lines. They resemble accurately the primary bundles of the neurilemma of the sciatic nerve and the rods of the retina of the healthy frog.

They stain deeply in gold preparations, and are then always puckered transversely.

In gold preparations of the inflamed frog's tongue, isolated primary bundles, identical in appearance and breadth with those of the inflamed cornea, are to be found.

The depth of staining by gold shows that the constituent elements of the primary bundles undergo a chemical change in inflammation.

The author has studied, by means of chloride of gold, the effects of inflammation in the quadrangular and in the long flat cells which cover the bundles in the interior of the cornea, but chiefly in frog-corneæ sealed

up in blood-serum, the latter method being found more certain to give available preparations.

The only appearance observed, anterior to a complete destruction of the cell, was a division of the nucleus into two or more parts. In serum preparations the products of the division assumed the form of circles of highly refractive particles. Similar particles were sparsely scattered in the substance of the cell.

The area of any one circular product of this division was always much smaller than that of the undivided nucleus.

In regard to the stellate cells, the author questions the correctness of the accepted theory, which implies an identity of the cell and its processes with the visible protoplasm. He considers that the refractive particles, which constitute what is visible in the cellular protoplasm, are suspended in a fluid, similarly to the pigment-granules in the pigment-cells as described by Mr. Lister. The phenomenon described by German investigators as "Zusammenballen" of the cell-processes, he attributes to a collection of the protoplasmic particles in the centre of the cell, similar to that which takes place in concentration of pigment. This opinion is borne out by a comparison of gold and osmic-acid preparations. In conditions in which, by the former process, an isolated globular body is seen, osmic-acid preparations show that the anastomosis of the thread-like processes remains complete. Reasoning analogically from the results obtained by gold in other tissues, he infers that it is what may be described as the contents of the cell and processes which stain by that method.

Treatment by osmic acid is the only reliable method by which he has obtained satisfactory preparations showing the stellate cells in the inflamed cornea. The advantages of this mode of treatment are much enhanced by subsequent staining with red aniline, which especially differentiates the protoplasm and processes. Subsequent staining by hæmatoxylin renders the nuclei visible.

The only change, except that of destructive disintegration, observed by the author as a consequence of inflammation in the stellate cells, consists in the anastomosing processes being, in gold preparations, occasionally represented by fine darkly stained lines, on which are a series of small globular swellings placed at short regular intervals, giving any one process an appearance identical with that presented by an ultimate nerve-fibrilla in a gold preparation. The same appearance is also to be seen in osmic-acid preparations, and is suggestive of points of communication between the lumen of the process and the interfibrillary space. (This is the only form in which the author has seen the processes of the stellate cells in inflamed corneæ in gold preparations. They are usually invisible by that process.)

Appearances indicative of a dividing nucleus were rarely seen, and their interpretation is doubtful. Both in respect to the nucleus and the

processes the stellate cells are the most stable of all the cellular elements of the cornea.

Between the layers of the superficial corneal epithelium a network of stellate cells can be seen in serum preparations of inflamed cornea. Indications of similar cells can be seen in gold and hæmatoxylin preparations of the healthy cornea.

In inflammation the cells of this network show a very great increase in size as compared with their appearance in health.

The changes produced by inflammation in the spindle cells may be divided into three stages:—

(*a*) Preparations examined in serum show that the cell-protoplasm has become increased in amount, and that the cell-processes can be distinctly traced. This stage can be observed after twelve hours' inflammation, resulting from slight cauterization in a winter frog. The swelling of the protoplasm is often confined to one or more tracts of the cornea, one of the above-mentioned clefts separating the area of this appearance from that of the normal cornea. The area extends from the neighbourhood of the cauterized part towards the limbus.

(*b*) The swelling of the protoplasm extends along the processes from one cell to the other, a chain of spindle cells being often represented by a long column of protoplasm on which there are very slight constrictions. This description applies to osmic-acid preparations. Deep staining with red aniline and subsequent treatment with acetic acid renders the nuclei visible in this protoplasmic column. This stage is well seen in osmic-acid preparations of a rabbit's cornea which has been 24 hours inflamed by the passing of a thread.

(*c*) With more or less increase in the amount of protoplasm, and with or without its presence in the processes in a granular form, nuclear bodies (resulting from a division of the nucleus) are seen in osmic-acid preparations to be contained in, or partly expelled from, the cell, which are identical in appearance with the red blood-corpuscles seen in the new vessels in the same preparations. This identity in appearance is further maintained by staining osmic-acid preparations with red aniline, in which the nuclear products and red blood-corpuscles are stained a like tint and deeper than the other elements. The author infers from these appearances that in inflammation the nuclei become free bodies, which are equivalent to red blood-corpuscles.

The appearances described by Key and Wallis, Cohnheim, and others as white corpuscles in "Spindelform," are seen in osmic-acid preparations to be spindle cells made more prominent by inflammation.

The "spiessartige Figuren" seen in gold preparations are produced by the protoplasm which immediately surrounds the nuclei of the spindle cells being visible, whilst from the mode of preparation the connecting processes are invisible.

White blood-cells in the inflamed cornea can be identified with most

certainty in osmic-acid preparations. They are found in groups in the wider spaces, in rows in the nerve-channels and between the primary bundles (corneal tubes of Bowman), and in large numbers in the tracts between the larger bundles. They are mostly round, sometimes club-shaped, never pointed at two extremities as an elongated shuttle-shaped mass (that is, never *spindelformig*, *spiessartig*). A small minority consist of a double body formed by two rounded globular masses joined by a smooth isthmus. When stained by hæmatoxylin, nuclei are found in either end, but not in the isthmus. The author infers that we have here a corpuscle in process of division.

In rabbit-corneæ, in which inflammation has lasted about a week, some white corpuscles are seen with uneven contour; and bulging outwards from, or lying close beside, them are bodies evidently nuclear, and which are affected by osmic acid and subsequent staining with red aniline, in a manner identical with the red blood-corpuscles seen in blood-vessels in the same preparation. The identity of the escaped nuclei with red blood-corpuscles is shown by a comparison of their respective size, evenness, colour, and contour.

The author infers a production of red blood-corpuscles in inflammation from the nuclei of the white blood-cells.

In observations on human blood, and that of the mouse, by staining with hæmatoxylin, he has found that while the great majority of the red corpuscles do not quickly stain in a weak solution, there are some which at once stain a deep blue, and that there are white corpuscles in which a narrow protoplasmic margin encloses a deep blue nucleus similar in contour and size to the stained red corpuscles. Amongst the red corpuscles of the frog are a minority which are recognized as being red corpuscles by their size, smooth contour, and absence of granulation, but in which there is no hæmoglobin, and the nucleus quickly stains blue in solution of hæmatoxylin, like that of the white cells.

Transitions occur in which a less and less capacity of staining on the part of the nucleus takes place, *pari passu* with an increase in the colour characteristic of hæmoglobin in the body of the cell. In the fully developed red corpuscle, the nucleus stains only after it has been for some time in contact with a weak solution of hæmatoxylin.

The author has observed in the blood of the mouse fœtus the nuclei of the nucleated red blood-cell escape from the larger cell, and then become indistinguishable in form and appearance from the small red corpuscles of the mature animal present in the blood under examination.

These observations, taken in connexion with the bodies that are formed in the spindle cells and white corpuscles in inflammation, support, as the author believes, the doctrine of Wharton Jones, in regard to the formation of the red blood-corpuscles.

The mode of formation of capillary blood-vessels he believes to be identical in inflamed and in fœtal tissue. In studying this subject he

has found special advantages from the use of osmic acid, with or without subsequent staining in hæmatoxylin. The stages in this formation are as follows:—

(*a*) The spindle cells enlarge and contain several nuclei which can be identified, whilst within the cell, as being of a similar nature to red blood-corpuscles. A current of blood-plasma from the nearest vessels passes, at the same time, into the interfibrillary space in which the spindle cells lie.

(*b*) The nuclei escape from the spindle cells into this space, where they are indistinguishable in appearance from the ordinary red blood-corpuscles.

(*c*) By a process of diapedesis the formed elements of the nearest blood-vessels pass into this space and the circulation is established.

Various appearances lead the author to suppose that the fibrine of the plasma solidifies on the outer surface of the current and forms the substratum of the new vessel, and on this substratum the white blood-corpuscles fix themselves and spread out as an epithelium.

From interfibrillary spaces in the inflamed cornea, in which formation of blood-vessels was actively taking place, the author has isolated white corpuscles in various transition stages towards the appearance and shape of epithelium; and, from rapidly enlarging vessels, cells which, from their form, he believes to be transitionary to that of smooth muscular fibre.

As the new capillary forms, the enlarged spindle cells decrease to their ordinary size.

In preparations of blood-serum of the frog sealed up, after a few days, the hæmoglobin may be observed to assume special forms inside the corpuscle, or to disappear from it, and so produce changes in the appearance of the corpuscle identical with those described by Arnold as taking place in the tongue of the living animal after diapedesis.

The above observations were made chiefly on the cornea of the frog and rabbit; and the inflammation was mostly produced by solid nitrate of silver, the passing of a thread, and the application of methylated alcohol.

In the winter frog (*Rana esculenta*), cauterized in the centre of the cornea, the first entry of white corpuscles attributable to inflammation was observed, after 48 hours, in the wider spaces near the limbus. After 4 days, they could be observed in considerable numbers, and 2–6 could be seen in one so-called space (*lacuna*).

II. "Report on Observations of the Transit of Venus made at Luxor, Upper Egypt, 9th December, 1874." By Vice-Admiral E. Ommanney, C.B., F.R.S. Received February 11, 1875.

Owing to the kindness of Professor Auwers, of Berlin, who placed his heliometer at my service, I was enabled to make the following notes of time and phenomena during the time of egress.

The time was given by a chronometer marked "Wiren 34," which was lent to me by the celebrated astronomer, William Döllen, of Pulkowa.

At $18^h\ 40^m$ M. T. the sun rose clear and brilliant over the eastern range of the Arabian hills on the valley of the Nile under very favourable conditions of sky and atmosphere, more so on this occasion than on any other morning during our stay of 20 days at Luxor. The first glance showed us the image of the planet Venus on the sun's disk in the predicted place, making progress in her path across the sun to the point of egress.

At the first observation the borders of the planet appeared jagged and ill-defined, but as the altitude increased she presented a dark disk, clearly defined on the sun. When the time of internal contact approached, the edge of the planet and the limb of the sun were both very distinct, and favourable for making accurate observations.

When the moment of internal contact drew near, I gave my utmost attention for observing the appearance of the black drop; but I could not detect it, though I could perceive with great nicety the instant of contact. The margin of the sun's limb and that of Venus were most clearly defined to my vision.

Immediately after internal contact a bright illumination manifested itself on the emerged part of Venus; this light continued bordering on the cusp for about three fourths of the time between internal contact and external contact at egress.

The following are the times shown by chronometer for contacts and phenomena:—

	h	m	s
Time at internal contact	20	01	02·5
Cusp of Venus illuminated	20	2	00
¼ Venus emerged, cusp illuminated..........	20	7	25
Light on right side of cusp became brighter ..	20	9	00
Light on left side became fainter............	20	15	00
Light at time of Venus's ½ emergence........	20	15	30
Illumination diminishing	20	17	00
Illumination disappeared	20	20	00
¾ Venus emerged	20	24	00
Time of external contact at egress	20	29	25

I must remark that I found it a matter of considerable difficulty to note the precise instant of the last or external contact at egress, as the indentation became so extremely slight towards the planet making final egress.

The error of the chronometer was estimated to be very nearly $+15^m\ 02^s{\cdot}0$ by preliminary calculation; hence the times of contact by my observations, corrected for mean time at Luxor, will stand thus:—

	h	m	s
Internal contact at egress	20	16	04·5
External contact at egress............	20	44	27·0

The temperature in the shade at sunrise was 53° F., and after transit 65° F.

The heliometer used by me on this occasion was constructed by Fraunhofer. One of the halves of the object-glass was used, the line of separation being put normal to the sun's limb at the point of contact, in order to produce the least distortion of image in the direction of the common diameter of the two objects. The focal length of the instrument is nearly 45 inches (English), the aperture 3 inches, and the power used was 97.

Our observatory was situated about half a mile to the southward of Luxor, in lat. 25° 41′ 40″ N., as determined by Wm. Döllen and Professor Auwers, and in longitude 2^h 10^m 22^s E., as fixed by Mahmoud Bey in his late survey of the valley of the Nile.

III. "Preliminary Abstract of Approximate Mean Results with the Invariable Pendulums Nos. 4 and 1821, in continuation of the Abstract published in vol. xix. of the Proceedings." By Captain W. J. Heaviside, R.E. Communicated by Professor Stokes, Sec. R.S. Received February 15, 1875.

Extract from a Letter from Captain Heaviside to Professor Stokes.

Dehra, N. W. P., 21st January, 1875.

My dear Sir,—An abstract of approximate results by the invariable pendulums was printed under Captain Basevi's superintendence in 1870. I now enclose an abstract in continuation, bringing the work down to Kew. The formulæ and factors employed by Basevi have been used in the reductions, so that the results in the two abstracts might be directly comparable.

The observations at Meean Meer and at Moré were taken by Basevi, and the reduction to mean sea-level for Moré has been computed in accordance with a memorandum he left, in which he assumed the mountain masses on which Moré stands to compose a cylinder, having a height of 2·92 miles and a radius of 200 miles.

You will see that the results at Kew, from my observations in 1873, differ by 0·38 vibration from those obtained by Mr. Loewy in 1866. My observations were taken in August, at a mean temperature of 65°; his in January, at a temperature of 54°.

As the temperature-factor (0·48 vibration for 1° Fahr.) here employed is larger than that which will eventually be adopted, the difference between the two results will be still further reduced, and the agreement will be much closer than I expected to obtain, when taking into consideration the varied travels these pendulums went through in the interval. * * *

Preliminary Abstract of Approximate Mean Results with the Invariable Pendulums Nos. 4 and 1821.

No.	Stations.	Geodetic coordinates.			Mean temperature.	Mean pressure.	Observed number of vibrations reduced to an infinitely small arc.	Corrections.		Corrected vibrations at the level of the station.	Reduction to mean sea-level.	Results.		
		North latitude.	East longitude.	Height in feet.				Reduction to 72° F.	Reduction to a vacuum.			By observation.	By computation in terms of Punnæ ellipticity $=\frac{1}{300}$.	Computed. — Observed.
						in.								
31.	Mian Mir (Meean Meer)	31° 32′	77° 25′	706	79°·0	1·50	86026·17	+3·35	+0·43	86029·95	+ 1·83	86031·78	86036·18	+ 4·40
22.	Moré	33 16	77 54	15427	55·0	1·25	85985·81	−8·10	+0·38	85978·09	+41·15	86019·24	86042·43	+23·19
...	Kaliána (1873)	29 31	77 42	826	62·3	2·97	86026·02	−4·60	+0·89	86022·32	+ 2·13	86024·44	86029·16	+ 4·72
28.	Colába	18 54	72 51	35	82·7	1·42	85995·03	+5·11	+0·41	86000·55	+ 0·09	86000·64	85997·66	− 2·98
24.	Aden	12 47	45 2	5	87·4	1·44	85979·55	+7·38	+0·41	85987·33	+ 0·01	85987·35	85984·88	− 2·47
32.	Ismaïlia (Egypt)	30 36	32 16	32	79·9	1·33	86027·23	+3·78	+0·38	86031·40	+ 0·08	86031·48	86032·90	+ 1·42
...	Kew Observatory (1873).	51 28	...	15	65·1	1·40	86117·22	−3·30	+0·42	86114·34	+ 0·04	86114·38	86113·55	− 0·83
...	Kew Observatory (1866).	...	...	...	54·2	1·57	...	−8·51	...	...	...	86114·03	...	...

Dehra, January 1875.

March 18, 1875.

JOSEPH DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On the Behaviour of the Hearts of Mollusks under the influence of Electric Currents." By Dr. M. FOSTER, F.R.S., and A. G. DEW-SMITH, B.A., of Trinity College, Cambridge. Received February 1, 1875.

It has already been shown by Dr. Foster (Pflüger's 'Archiv,' vol. v. p. 191) that an interrupted current applied directly to the heart of the common snail (removed from the body) will cause an arrest of the rhythmic beat, the heart remaining *in diastole.*

The phenomena thus produced are altogether similar to those seen when the vertebrate heart is inhibited by stimulation of the pneumogastric nerve.

This inhibition by direct stimulation requires a current weaker than is sufficient to cause tetanus of the heart. It may be witnessed on either auricle or ventricle, and is the more easily obtained the more quickly the heart is beating. It takes place whatever be the position of the electrodes; and we find that it is possible thus to inhibit not only the whole ventricle but separate parts of a divided ventricle, provided these can be obtained, as they sometimes can, beating with a sufficiently rapid rhythm.

We have found no reason to think that there exists any localized inhibitory mechanism (corresponding to the sinus venosus ganglia of the frog's heart, for instance) any more than a localized automatic mechanism.

This inhibition by direct stimulation is, to say the least, an unusual fact; and the following inquiry was begun in the hope that further study of the heart of the same animal, and those of other mollusks, might bring to light some explanation of the matter.

We will relate first the results of our experiments with single induction-shocks and with the constant current on the snail's heart, and then pass on to the facts observed in other mollusks.

We have in all cases removed the heart from the body, and placed it in a watch-glass, in a little snail's blood. It will in this condition continue to beat with great regularity for hours.

Our reasons for always removing the heart from the body are as follows:—

The movements of the heart cannot be accurately observed *in situ* unless the overlying thin integument is divided and the pericardial chamber laid open. As soon as this is done the beats are seen to become very irregular. Every movement of the body influences the flow of blood to and from the heart, and every marked change in the flow affects the force and rhythm of the beat; hence observations like those we are about to record would be impossible with the heart *in situ*.

We have been unable to find any nerve in the body stimulation of which would inhibit the heart in the same way as stimulation of the pneumogastric does the vertebrate heart.

By removal of the heart from the body, therefore, we lose nothing essential.

A graphic record of each heart-beat would of course be impossible. In order to obtain a satisfactory register of our observations, we adopted the following plan. One of us acting as observer, and watching the heart through a microscope (50 to 100 diameters), signalled, by the sudden make and break of a constant current, each beat of ventricle or auricle, or of both. The make and break were recorded by a magnetic marker, as was also the application of the stimulus, on a travelling-paper, on which seconds were at the same time being marked. In this way a little practice enabled us to register with tolerable accuracy the rhythm of the beat, though we could not, of course, record the force or character of each pulsation.

Most of our observations were directed to the ventricle, and in our remarks we have only incidentally referred to the auricle.

We at first registered the auricular beats as well as the ventricular; but an increasing conviction of the independence (in an empty heart) of the movements of the two chambers, led us after a while to confine our attention to the ventricle.

The application of the current was made by one of us while the other, watching and recording the beats of the heart, was unaware of what was being done. The faithfulness of the observations was thus materially assisted.

Finding the heart of the common snail more vigorous, though smaller, than that of the edible snail, we confined our attention to the former.

The snail's heart consists, as is well known, of a thin-walled globular auricle, separated by a ring of non-contractile tissue from an oval, almost conical, fleshy ventricle. Into the former, opposite the auriculo-ventricular orifice, opens the large pulmonary vein. The latter at its apex, opposite the auricle, narrows to the large artery, which we have spoken of as the aorta. In speaking of the ventricle we shall frequently have occasion to distinguish the broader auricular end next to the auricle from the narrow aortic end, which is continuous with the aorta. In

removing the heart from the body, it is of importance, in order to secure a good rhythmic beat, that the incision should be carried through the aorta, and not invade the contractile tissue of the ventricle itself.

While the auricle is a sac, with quite thin and smooth walls, the bundles of fibres in the ventricular walls bulge out largely into the cavity, and are so arranged that the ventricle has the same spongy structure as that of the frog and many other animals.

Neither in the auricular nor ventricular wall can the presence of any nerve-cells, or collection of nerve-cells in the form of ganglia, be detected, whether in fresh specimens or in those treated with various reagents. The interlacing intricately arranged bundles of fibres are composed of a granular protoplasmic-looking tissue, quite unlike the ordinary muscular tissue of the body, and in many ways resembling the cardiac tissue of vertebrates.

The walls are thickly studded with nuclei, some of which possibly belong to an external tesselated epithelium. Other nuclei are undoubtedly the proper nuclei of the contractile elements, and the remainder seem to be of the nature of connective tissue.

Of none of them can it safely be said that they are the nuclei of nerve-cells. Molluscan nerve-fibres might undoubtedly, unlike vertebrate medullated nerve-fibres, easily escape detection; but Mr. A. S. Lea, of Trinity College, carefully examined for us the whole of both the auricle and ventricle without discovering any distinct nervous structures. He also went systematically over the margins of both the aortic and pulmonary orifices, but could find no nerves running into or out of the heart. In no other way could nerves become connected with the heart. And, opposed as it may seem to general experience, and still more to recognized opinions, we are led to the conclusion that the heart of the snail has no nervous connexion with the rest of the body; nay, more, that it has within itself no distinctly specialized nervous mechanism, but that its contractile elements are composed of protoplasm, arranged, it is true, more or less in fibres, yet otherwise but slightly advanced in differentiation.

The Effects of single Induction-shocks.

A single induction-shock sent through the ventricle of the snail's heart produces different results, according to the strength of the current employed. We have generally used the closing shock alone, or the opening shock alone, stopping the other shock in the usual way. Except that the opening shock acts, of course, as a stronger stimulus than the closing shock, we have not observed any marked difference between their effects.

When the shock is of a certain intensity (as, for instance, when a single Daniell's cell is used with the secondary coil of a Du Bois Reymond's induction-machine drawn out to 5 or 10), its application is immediately followed by a contraction. This contraction has all the characters of a normal beat, except perhaps that it is rather more sudden and of shorter

duration. Since the eye has been our only means of judging of the characters of the contraction, it being impossible to register the movements by the graphic method, we speak with great uncertainty on this point.

The stronger the shock the more marked, within certain limits, is the contraction. The effect, of course, in any particular case will depend not only upon the absolute strength of the current, but on its strength in relation to the irritability of the heart. Thus a heart with frequent and strong beats is affected by a weak current to the same extent as is a heart beating slowly and feebly by a strong current.

The contraction or beat thus produced by a single induction-shock is generally followed by a pause, much more evident in some cases than in others. We have not been able to discover the exact conditions which determine the duration of the pause; but it seems to depend much more on the state of the heart than on the strength of the current. There appears to be no necessary relation between the amount of the contraction and the length of the consequent pause. Thus a contraction twice the strength of the normal beat is not invariably followed by a pause of twice the length of the normal diastole; it may be more than twice as great, it may be hardly greater. On this point, again, we speak with hesitation, from inability to measure accurately the force or extent of the beats.

We have thrown in the single induction-shock at all phases of the cardiac cycle, at the height of systole, immediately before and immediately after systole, in the middle of diastole, &c., but have been unable to detect any marked differences in the result. It can hardly be doubted that, whatever be the way in which the shock causes a contraction, the heart must be differently disposed towards a stimulus, according as it is about to make or has just made a beat; nevertheless we have not succeeded in observing any marked differences.

Not only has the contraction produced by a single induction-shock the general characters of a normal "beat" rather than those of a simple muscular contraction, but, as far as can be judged by the eye, the effect of the shock on the ventricle differs from the effects produced in any ordinary muscular fibre in the following respects. In passing gradually from extremely feeble to stronger currents, there is not witnessed a gradual increase of effect; on the contrary, the change is a sudden one, from apparently no effect at all to the production of a well-marked beat. This leads us to infer that the mechanism by which feeble currents produce contractions in the heart differs somewhat from that whereby contractions are caused by stimulating an ordinary muscular fibre *. The stronger contractions, on the other hand, produced by stronger shocks, seem to share more distinctly in the characters of an ordinary muscular contraction.

* Compare Bowditch, Arbeiten phys. Anstalt, Leipzig, vi. Jahrgang (1871), p. 139.

We have said that single induction-shocks, below a certain intensity, produce apparently no effect on the heart.

We have thrown in single shocks, of a strength too slight to cause a beat or contraction, at all phases of the cardiac cycle, without being able to detect any variation in either the character or frequency of the beat. We would not venture to say that absolutely no effect is produced, but the effect is not manifested by any change in the rhythm which can be measured on our recording surface, travelling half an inch a second, or by any change in the beat which can be recognized by the eye.

We have also thrown in single shocks repeated at the same phase for several beats in succession. Thus we have thrown in a single weak shock at the height of systole as well as in the middle of diastole, during several successive beats, without detecting any appreciable effects.

Very marked, however, are the results when the weak single induction-shocks are rapidly repeated, so that two or more shocks fall within each cycle.

The application of the shocks produces no contraction or beat, but is followed by a prolongation of the diastole.

In other words, a single induction-shock, too weak to produce by itself any effect, has, when repeated within the cardiac cycle, a distinct inhibitory action.

The beat which immediately follows the prolonged diastole is a feeble one (as are also the beats which occur when the shocks are repeated many times in succession, so as to cover the periods of more than one prolonged diastole); but the inhibition is followed, as was the case with the interrupted current, by a stage of increased rapidity and vigour of beat—that is to say, the inhibition thus caused has all the characters of a normal inhibition.

Repeated trials have shown us that, having due regard to the irritability of the heart, the more feeble the current the more rapid must be the repetition of shocks, and the greater the number of shocks given in order to produce inhibition.

As an instance of very successful inhibition, we may refer to fig. 1, (p. 342), where two shocks were found sufficient to produce a very marked prolongation of the diastole.

A particularly interesting result is shown in fig. 2. This is the only case in which we succeeded in obtaining inhibition as the result of one single opening shock. The shock, however, was (in relation to the heart's irritability) a comparatively strong one; it was just on the verge of being strong enough to produce a contraction. Our notes record that at the moment of stimulation (the stimulus being thrown in very soon after the normal beat) "a slight quiver, quite distinct from any ordinary contraction, and limited to the neighbourhood of the electrodes, was observed."

It will be noticed that after the first two shocks the heart became habituated to the stimulus, and the last two shocks produced no effect.

The "slight quiver" recorded in the above observations does not really contradict the statement made above concerning the rapid transition from a state of rest to a full though slight beat. The transition from this quiver to a beat is quite as abrupt as that from a state of complete rest.

As far as we have seen, inhibition may be brought about whether the electrodes be placed lengthwise (one at the auricle and the other at the aorta) or sideways, or obliquely.

We have not, however, paid particular attention to the question, whether inhibition can be produced more easily in one direction than another; nor did we, in making the above observations, pay attention to the direction of the current.

It would seem, then, from the facts we have recorded, that the inhibition produced by the tetanizing current, applied directly to the ventricle of the snail's heart, is in reality the simple summation of the effects of the single induction-shocks which make up the tetanizing current.

The effect of each shock is very slight, and a single shock has rarely the power to cause a marked prolongation of the diastole; nevertheless there is an effect which lasts for some time longer than the application of the shock itself, and of such a kind that a second shock being brought to bear on the heart before the effect of the first has passed away, has a cumulative effect, and, in consequence, distinct inhibition is observed, if not always with two, at least with several shocks rapidly repeated.

Moreover this inhibitory effect, this prevention of the contraction or beat, increases with the strength of the current employed up to a certain limit, and then the action of the stimulus is suddenly reversed, and a contraction or beat is caused instead of prevented.

There seems to us to be no escape from this conclusion, that the action of the induction-shock on the tissue of the heart is of such a kind that, beginning with a certain effect (inhibition), as it increases in intensity it suddenly topples over, so to speak, and produces the directly opposite effect (contraction).

We cannot be assisted here by the favourite theory of a double mechanism of inhibition and contraction, with different exhaustibilities, even if we suppose that these mechanisms are evenly distributed all over the ventricle. All exhaustion is, as far as we know, gradual. If we suppose the inhibitory mechanism to be more easily excited and exhausted than the contractile, then in passing from weaker to stronger stimuli we must pass through a phase in which the waning inhibition is just sufficient to counteract the increasing contraction, when, consequently, no effect at all will be produced. In our results, on the contrary, we pass at once from the *maximum* of inhibition to contraction.

The Effects of the Constant Current.

Non-polarizable electrodes (a modification of Donder's pattern) were

always used. The current was supplied by a single Daniell's or Grenet's cell, the circuit being divided between the electrodes placed on the heart and a set of resistance-coils, the resistance in which could be made to vary from ·05 ohm to 100,000 ohms, and thus a very great variety obtained in the strength of the current passing through the heart.

It will be as well to begin by describing the effects produced by the constant current on the heart at rest, *i. e.* on hearts which, after removal from the body, did not exhibit any spontaneous beat.

When the ventricle alone was experimented with (the auricle having been removed), and the electrodes placed lengthwise, one against the auricular end, the other against the aortic end of the ventricle, a contraction or beat always took place at the making and at the breaking of the circuit; but the two contractions differed from each other and from a normal beat in a very distinct manner.

The normal beat of the ventricle may be described as a gathering of the whole mass towards the point where the long axis of the ventricle would be intersected by a transverse line drawn across the ventricle's broadest part, accompanied by a kind of peristaltic constriction, which travels down from the auricular to the aortic end, *i. e.* which begins at the auricular and leaves off at the aortic end, though during the greater part of the period of the beat the whole of the ventricle is constricted. These features of the beat may be recognized in an ordinary beat, and become very obvious when the beat is abnormally prolonged, as when under the influence of certain drugs (atropine &c.). From this normal beat the beats or contractions caused by the closing or opening of a constant current differ in the following way.

The closing- or making-beat always starts from the kathode, and travels for a variable distance towards the anode; the opening- or breaking-beat always starts from the anode, and travels towards the kathode, the beat in each being a kind of peristaltic contraction, the features of which are more readily recognized when it starts from the aortic end.

This kathodic making-beat and anodic breaking-beat make their appearance with all intensities of current up to the full strength of a Daniell's cell (beyond this we have never gone), whatever be the position of the electrodes. When the kathode is at the aortic end, the beat begins there; when at the auricular end, the beat begins at the auricular end, and in that case differs chiefly from the normal beat in being more or less confined to that end. When the electrodes are placed at the sides of the ventricle, the kathodic side beats at the making, and the anodic at the breaking, whichever side each be placed.

We have not made many observations on the auricle alone, but have seen enough to satisfy ourselves that the same law holds good there too, that the making-beat is kathodic, and the breaking anodic.

We need hardly point out the remarkable analogy presented by these facts with those resulting from the passage of a constant current through

a nerve, and the support they thus afford to Pflüger's hypothesis, that the establishment of kathelectrotonus and the disappearance of anelectrotonus alone set in motion a stimulus-wave.

When the ventricle and auricle are removed together from the body, and placed between the electrodes, so that one electrode lies at the aortic end of the ventricle and the other at the pulmonary end of the auricle, the ventricle behaves, as far as the make- and break-beats are concerned, very much as if the electrodes were applied directly to the ventricle alone. Thus when the kathode is at the auricle there is a "making"-beat at the auricular end of the ventricle and a "breaking"-beat at the aorta, and *vice versâ.*

We have satisfied ourselves by numerons observations that, provided the contraction of the auricle causes no *distension* of the ventricle, the condition of the one chamber of the heart has no effect on that of the other. Between the two is a small ring of a sort of connective tissue, traversed, as far as we can make out, by no nerves, which affords complete physiological isolation of the two chambers. Thus out of the body, empty of blood, auricle and ventricle are two independent organs. Hence when a current is passed in the manner described through both ventricle and auricle, each part undergoes a separate *physiological* polarization, the auricular end of the ventricle becoming kathodic or anodic as the case may be, just as if the current were applied to the ventricle alone, account being taken of the weakening of the current through the additional resistance offered by the auricle. This, at least, is the only explanation we can give of the undoubted fact, that, with some modifications to be mentioned afterwards, the effect of the constant current is the same on the ventricle with the auricle attached as on the ventricle alone.

Besides the mere occurrence of the kathodic making- and the anodic breaking-beat, there are several facts worthy of attention. These we have studied chiefly on the ventricle with the auricle attached; but we see no reason to doubt that the same facts appear when the ventricle alone is used.

When the period during which the current passes is very short, when the current is very rapidly made and broken, the two contractions are replaced by one. This contraction or beat begins at the kathode, *i. e.* is aortic or auricular according to the position of that electrode.

Here, again, is a curious parallel to the view so generally adopted in reference to the behaviour of nerves towards the constant current, viz. that the establishment of kathelectrotonus is a stronger stimulus than the disappearance of anelectrotonus.

When exceedingly feeble currents are employed, it frequently happens that a momentary application of the current, a rapid make and break, produces no contraction or beat. Nevertheless such a current, if applied for several seconds, will give, as usual, a kathodic making- and an anodic breaking-beat.

The absence of a beat on a momentary application might be explained by supposing that the effect of the too rapidly succeeding break interferes with the effect of the immediately preceding make. But, on the one hand, we have no positive indication of the possibility of such an interference (and there are reasons for thinking it extremely unlikely), while the normal character of the kathodic beat, when it does occur with a stronger current, distinctly opposes such a view. On the other hand, a distinct interval does exist between the making of the circuit and the commencement of the contraction, an interval easily recognizable by the eye, though, in the absence of any graphic method, we have been unable to measure it. We conclude, therefore, that, in order to produce the maximum effect of even the making kathodic beat, a certain time is required. This time is longer for weak currents than for strong; and currents sufficiently weak may be applied for a short time without producing any beat, the maximum effect not having time to develop itself, though the beat does appear when the action of the same current is prolonged.

Sufficient time being allowed for the production of the beat, the strength of the kathodic making-beat will depend otherwise entirely on the strength of the current (the irritability of the tissues of the ventricle being supposed to be constant).

The strength of the anodic breaking-beat depends, however, in a peculiar way on the duration of the current. Up to a certain point the more prolonged the action of current the stronger is the anodic beat; but a limit is soon reached, after which the strength of the anodic beat is diminished by further prolongation of the action of the current. We have not gone into this point fully, but the result is evidently connected with a so-called "exhaustion" of the tissue.

We have called the contraction which takes place at the make or break "a beat." We think we are justified in giving it this name, though it differs, as we have pointed out, from the normal beat in some particulars. Our strongest reason for doing so is as follows:—

When the ventricle and auricle are placed between the electrodes, so that the kathode is against the auricle and the anode against the aortic end of the ventricle, the kathodic making-beat is *followed by a second, by a third, and by sometimes several beats,* all kathodic in their features, *i. e.* starting from the kathode and travelling towards the anode. In other words, a rhythmic pulsation is set going at the kathodic end.

It is impossible to regard these beats as simple contractions caused by the stimulus-wave, started, like the stimulus-wave in a nerve, by the establishment of kathelectrotonus. They are evidently actual *beats,* whose occurrence is favoured by the conditions brought about by the action of the current (fig. 10, p. 343.).

Of these the first (the ordinary making-beat) is always (as far as we have seen) the strongest. The succeeding ones diminish in intensity,

and the intervals between the beats generally become increasingly prolonged (see fig. 13).

When the current is maintained for a short time only (10 seconds), the appearance is produced as if the heart were beating during the whole time of the passage of the current (fig. 11).

The anodic breaking-beat is, as far as our observations go, always single, is never followed by a series of beats (that is, of course, when the ventricle is one which is incapable of spontaneous pulsation).

We conclude that the constant current throws the tissue in the neighbourhood of the kathode into such a condition as is favourable for the development of beats; that this effect, though taking some time for its production, reaches its maximum very soon after the making of the circuit, and thenceforward diminishes gradually.

In the neighbourhood of the anode, on the other hand, the tissue is thrown into a condition unfavourable for the production of beats, the beats which have originated in the kathodic region ceasing as they approach the anode. The rebound, however, which follows upon the breaking of the circuit develops a beat, with the occurrence of which the tissue returns to a normal condition of equilibrium, and no further beats occur.

Such are the facts which may be observed when the kathode is placed against the auricle, and the anode against the aortic end of the ventricle.

One would naturally expect that similar results, *mutatis mutandis*, would be obtained when the kathode was placed at the aortic end of the ventricle, and the anode at the auricle. Such, however, is not the case.

When the kathode is at the aorta there is only a kathodic making- and an anodic breaking-beat. Between the two the ventricle, as a general rule, remains perfectly quiet (see figs. 12 & 9). There is therefore a functional difference between the auricular and aortic ends of the ventricle.

Led by the apparent analogy of the vertebrate heart, one might be inclined to infer that there was an automatic mechanism present at the auricular end of the ventricle, but none at the aortic end.

This view, however, is directly negatived by the following facts:—

The aortic end will, in active and favourable hearts, continue to beat spontaneously for some time after being separated from the rest of the ventricle.

When, as we shall presently have occasion to mention, a spontaneously beating ventricle is submitted to the action of the constant current, the beats begin at the aortic end, and are frequently entirely confined to the aortic end when that end is made kathodic. This would be impossible if the automatic mechanism were confined to the auricular end. We have been led to connect the explanation with the shape of the ventricle

The greater mass of the contractile tissue of the ventricle lies towards the auricular end. If the ventricle were supposed to be bisected transversely by a straight line drawn across it, and its constituent fibres regarded as so many straight lines, representing so many forces, those forces might be considered as applied at a point on the auricular side of that straight line.

If we, then, suppose (we shall presently state our reasons for such a supposition) that the rhythmic impulse is generated by changes in all the fibres, that impulse would naturally manifest itself first, as in fact it does, at the auricular end.

Moreover, since the heart was first formed, each beat has been taking place after a fixed normal fashion, beginning at the auricular end. Hence the nutrition, the life (let it be called what it may) of each part is habituated and regulated to such conditions as are involved in the normal beat beginning at the auricular end. One would therefore expect that, even under abnormal conditions, it would be easier to call forth a beat beginning at the auricular end than at the aortic end, and that when the heart is divided into two pieces the auricular end would manifest a rhythmic beat more readily than the aortic end, though each end were absolutely possessed of the power of rhythmic pulsation.

This is actually the case. The aortic end, separated from the auricular end, will, under favourable circumstances, beat spontaneously, but will not do so with the same readiness as will the auricular end. Under the influence of the constant current, the beats of a spontaneously beating heart may be made to begin at and be confined to either the aortic or the auricular end, but not indifferently; they appear most readily at the auricular end. Under the influence of the constant current, the heart, otherwise at rest, may be made to beat spontaneously at the auricular end, but not at all, or with great difficulty, at the aortic end.

So far concerning the action of the constant current on the ventricle at rest.

We have already been led to anticipate some of the results gained by applying the constant current to a heart spontaneously beating.

These are somewhat more difficult to analyze, on account of the greater susceptibility of the tissue, and the greater variations in irritability resulting from treatment.

Thus, when the current used is one so strong that the momentary application of it is followed by a powerful contraction, it is evident that in dealing with the effects of the same current applied for several seconds, we have to consider that the powerful initial " making " contraction must influence, by the reactions following it, the behaviour of the heart during the passage of the current. In the same way the breaking of the current will be followed by events the causes of which are sought for partly in the reaction following upon the strong contraction caused by the break.

To avoid these difficulties we have generally employed a current so weak, that a momentary application of it to the beating heart produced no appreciable result.

During the passage, however, of such a weak current very marked effects make their appearance.

Whatever the position of the electrodes, whether the current be directed longitudinally, with the kathode at the auricular end or at the aortic end, or transversely or obliquely, in all cases the normal beat is modified in such a way that the contraction begins at the kathode, and is more or less limited to the neighbourhood of the kathode. Thus, when the kathode is placed at the auricular end of the ventricle, the part immediately round the kathode contracts first, and the contraction passes down in a vermicular manner towards the anode, sometimes a very little way, sometimes a considerable distance. When the kathode is placed at the commencement of the aorta, the contractions begin at the aorta and pass upwards towards the auricular end.

Being thus partial, the beats may be said to be weaker than normal, the more so the more they are confined to the kathode. Their feebleness seems to be more evident with stronger currents (not so strong, however, as to produce an initial contraction) than with weaker ones.

We have not been able to satisfy ourselves as to any marked or constant change in the rhythm taking place.

These kathodic beats are succeeded, on breaking the current, by one or more markedly strong beats of a reverse direction, beginning at the anode and travelling towards the kathode.

In some cases the reversal has been seen to take place during the passage of the current, the last few beats beginning at the anode, but not then exhibiting the same increase of strength which is seen in the anodic beats succeeding the breaking of the current.

This last observation illustrates the fact that the effect of the constant current, like that of the tetanizing current, is at its maximum effect near the commencement, and thence declines with greater or less rapidity, the tissue becoming insensible to the current. In this case insensibility was reached after the current had been passing a very short time; and it is interesting to remark that, though the direct effect of the current thus became lost, the reaction caused by the action of the current in the first part of the period was able to manifest itself by the anodic beats.

Thus, like the initial and final beats observed when the ventricle is at rest before the application of the currents, the apparently spontaneous beats always originate at the kathode, are more or less confined to the kathodic region, and, according to the position of the electrode, may be witnessed either at the auricular or at the aortic end.

As with the heart at rest, however, so with the heart in spontaneous movement, there is a functional difference between the aortic and the auricular ends.

This difference is most readily observed when the ventricle with the auricle attached is placed between the electrodes than when the ventricle alone is subjected to the action of the current, though we have observed the same phenomena in the latter case also.

When the anode is placed at the aorta, and the kathode at the auricle, a very feeble current produces no appreciable effect, except that perhaps the beats are rather feebler and somewhat quicker (fig. 5, p. 342).

The same current, applied to the same heart in the opposite direction (that is, with the kathode at the aorta and the anode at the auricle), *distinctly inhibits the ventricle*, so that it remains in diastole during the whole time of the passage of the current, and resumes its beat on the current being broken.

This inhibition is very clearly shown in fig. 7. Here advantage was taken of a curious secondary rhythm (fig. 6), which we have had more than one opportunity of witnessing. The ventricle, after a period of quiescence, began to beat, at first feebly with long pauses, then more rapidly and strongly. Having reached a maximum, the beats similarly declined, and thus a new period of quiescence was again begun. This alternation of rhythmic beats and quiescence was observed for a long time.

It will be seen that when the constant current (which was exceedingly weak, inasmuch as the resistance-circuit only offered a single ohm, or even only a fraction of an ohm resistance) was thrown into the heart at one of the ventricle's beating periods, no effect was produced when the anode was at the aorta, but distinct inhibition when the kathode was at the aorta.

During the period of quiescence the effect of the current is very much what we have already described as that of the constant current on a ventricle which has ceased beating, viz. that when the anode is at the aorta, and when, consequently, the auricular end of the ventricle becomes kathodic, a rhythmic beat tends to make its appearance, but when the auricular end is made anodic, no beats make their appearance (fig. 8).

The explanation we would give of these facts (very similar to that given at p. 328) is as follows:—

The current employed was a very feeble one, rendered all the more feeble, as far as its effect on the ventricle was concerned, by the fact of some of it having been occupied in effecting the polarization of the auricle.

When the kathode is placed at the aorta, the auricular end of the ventricle becomes anodic. Hence, as we have already seen, the beats would, if the current were of a sufficient intensity, while absent at the auricular end, be present at and start from the aortic end. We have already seen that the auricular end is more sensitive than the aortic end. Hence, with a sufficiently weak current, as in the case in point, the auricular end being more affected than the aortic end, the beats are inhibited at the auricular

end while as yet the current is unable to produce the beats at the aortic end, and the result is an apparent general inhibition of the whole ventricle.

The inhibition thus produced is, as far as we can see, a distinct inhibition; that is, the heart, which previously was beating regularly, stops beating when the current is thrown into it, remains in diastole during the short time the current continues to pass through it, and resumes its beat on the current being removed.

The occurrence of inhibition is, then, here a matter of degree; it depends on the existence of a certain relation between the irritability of the ventricle and the strength of the current employed. When a stronger current is employed, the beats, while continuing to be absent at the anode, make their appearance at the kathode, and the heart, though beating quite differently from the normal, can no longer be said to be inhibited. There is no room here for any theory of a special inhibitory mechanism, except such a one as would suppose that while the automatic mechanism was exalted at the kathode and depressed at the anode, the inhibitory mechanism was by weak currents exalted at the anode and depressed at the kathode. Such a view would be either simply a clumsy expression of facts or, if any thing more, directly opposed to all our experience of the behaviour of irritable living matter towards electric currents.

We would now call attention to fig. 5. This is the same heart which was inhibited by a current passing from the auricle to the aorta, and is apparently but little affected by nearly the same strength of current passing from the aorta to the auricle. But it will be noticed that, though the throwing of the auricular end of the ventricle into a kathodic condition does not very much, if at all, increase the beat of the ventricle, the withdrawal of the current is followed by a very distinct pause (a)—in fact, by an inhibition of short duration.

This pause must be due to a reaction taking place in the kathodic region. It can hardly be due to a reaction taking place at the anodic or aortic region; for, as we have already seen, reaction at the anode takes on the form of a beat or contraction. We thus get this remarkable result, that at the kathode, where the action of the current during its passage is favourable to the beat, *the after effects, which are in the way of inhibition, are more marked than the effects of the current itself, which, as far as they go, are in the way of a quickening of the beat.*

In other words, at the auricular end of the ventricle, of whose condition, by reason of its greater susceptibility, we are better able to judge than of that of the aortic end, we find that, whatever be the direction of the current, the total effect of the passage of the current is inhibitory. For when the auricular end is anodic, the current produces on that part of the ventricle a direct inhibitory effect, which the exalted (kathodic) condition of the aortic end is unable to counterbalance; and when the auricular end is kathodic, the depressed (anodic) condition of the aortic

end hinders any great increase in the total force of the beats—so that the direct effect of the current is manifested only, if at all, in a quickening of the beat, while the after effect is distinctly inhibitory, to such an extent as not to be obscured by the reaction, in the way of exaltation, which takes place at the (anodic) aortic end. In other words, whichever its direction, in the one case by action, in the other by reaction, a sufficiently weak current leaves a *balance* in favour of inhibition.

Though, by reason of the less susceptibility of the aortic end and the consequently slighter development in that region of a rhythmic beat, we are less able to judge of what is going on in that region during the passage of the current, we may, we venture to think, assume that the changes there are of fundamentally the same kind as at the auricular end—that is to say, that the total effect of the passage of the current would at this end too be greater *in the direction of* inhibition than of exaltation. A sufficiently weak current, then, passed lengthwise through the ventricle in either direction, whether from aorta to auricle or from auricle to aorta, has an inhibitory effect (when the period of reaction is included) greater than its exalting effect.

If we suppose such a current to be momentarily passed through the ventricle between any two beats, its effect therefore, as far as it went, would be to prolong the diastole. The effect might not be very obvious; it might be so small as to escape detection by itself, but it might be made manifest in the following way:—The effect of the current, as we have in all cases seen, whether exalting or inhibitory, takes some time to establish, and lasts longer than the actual passage of the current. Hence, if a momentary current like the above be succeeded before the next beat by a second similar one, the effect of the second would be added to the first, and the diastole still further prolonged. By means of momentary currents, repeated so rapidly that the effect of one had not passed off before the next began to tell, the diastole would be so prolonged that the inhibitory effect of the current would be undeniable.

Moreover, though we have used the longitudinal application of the current as a means of analyzing its effects, there is nothing to prevent a similar explanation being given of the inhibitory effects of a weak momentary current passed transversely, or in any direction, through the ventricle or through the auricle. The inhibitory effect of the longitudinal current depends essentially on the fact that the total effect of the current, when the after effects are included, is one of depression, though it has partial elements of exaltation; and this will hold good whatever be the position of the electrodes, provided that the current be of a certain intensity in relation to the irritability of the tissue.

We conclude, then, that the clear and obvious inhibition brought about by sufficiently rapid repetitions of momentary currents (such as single induction-shocks) of a certain feebleness is due to the summation of a number of slight inhibitions, the occurrence of each single inhibition

depending on the fact that the total depressing effect of the current on the rhythmically beating tissue is greater than the total exalting effect.

It may seem strange to speak of a stimulus as producing a depressing effect; but reflection will show that it is quite what ought to be expected.

We have in the foregoing contented ourselves with speaking of the tissue in the neighbourhood of the electrodes as being in a kathodic or anodic condition as the case may be. We did so because, not having examined the condition of the electric currents of the ventricle, we hesitated in using the terms kathelectrotonus and anelectrotonus.

We know, however, that muscle is capable of being thrown, *in the intrapolar region*, into an electrotonic condition, and that (quite in accordance with all the results recorded above) this condition lasts after the removal of the current. We therefore shall probably not err in supposing that the polarization of the cardiac tissue, which is obviously a result of the constant current, is more or less allied to that kind of polarization which we call electrotonus.

Now the electrotonic condition of nerve or muscle differs essentially from the polarization of any dead matter in this, that it is *essentially a function of the vital activity* of the substance polarized. May we interpret this as meaning that the change in muscle or nerve which constitutes the electrotonic condition is brought about and maintained, not solely at the expense of the energy of the current, but also, perhaps chiefly, *at the expense of the energy of the tissue itself*?

If so, then we may argue that while in the normally beating heart all the energy of the tissue is used for the production of the beat, when a constant current is passed through it some of this energy, in addition to the energy of the current itself, is used in establishing anelectrotonus and kathelectrotonus. There must therefore, as the result of the polarization, be less energy (for the time being) available for the purposes of pulsation.

The idea of the exalting influence of a stimulus arises naturally from the fact that the stimulus may, if sufficiently intense, give rise to a contraction; and it seems at first sight contradictory that one and the same thing should produce apparently exactly opposite effects.

It is very easy, however, to trace, in the beating ventricle of the snail's heart, the transition from inhibition to the production of a contraction or artificial beat.

One stage in this transition is shown in fig. 4.

Here a weak constant current passed through the ventricle longitudinally from auricle to ventricle (and thus most favourable for inhibition) caused, when the passage was momentary (*i. e.* was removed before it had time to produce its full effect, and thus was reduced, so to speak,

in its action much below its normal strength), simply a brief inhibition, indicated by the greater length of the diastole marked *a*.

The same current, allowed to act on the tissue a sufficient time for it to produce its maximum effect, gave rise to a making- and breaking-beat, the making-beat being what we have called kathodic (starting from the aortic end in this case), the breaking anodic (starting from the auricular end). The current was sufficiently strong while inhibiting the auricular end to cause a kathodic beat of the aortic end.

This same current would, if sufficiently increased in strength, have been able to produce, on a momentary passage, the same effect as it has in the above observation by prolonged passage, with this exception, that there would be one visible beat only instead of two.

In this way, by simply varying the intensity of the current, we get either inhibition or contraction.

(It will be observed that in fig. 2 the kathodic beat is followed by a pause, as is also the anodic beat; and a single contraction produced by the momentary application of a stronger current would show a similar pause. These pauses may be regarded as partly due to the effects of the contractions—that is, to what is commonly spoken of as exhaustion; but it is obvious, from all our preceding observations, that they are partly due to the direct inhibitory effect of the current.)

There is, then, no absolute contradiction in the fact of a current producing either inhibition or a beat, according to its intensity; and even when a beat does take place, the essentially depressing effect of the current is seen in the subsequent pause or inhibition, generally spoken of as exhaustion.

With the occurrence of a contraction, however, the direct effects of the current become mixed with the effects and after effects of the changes resulting in the shortening of the fibres; and thus the pause after a contraction has laws of its own, more or less different from those of the simple inhibitory pause, and is more difficult to analyze.

On a Secondary Rhythm.

We would wish to call attention to the peculiar secondary rhythm, an example of which is shown in fig. 6. The characters of the rhythm are very marked. The heart, after remaining for some seconds in an apparently perfectly quiescent state, gives a beat hardly recognizable even under the microscope. This is succeeded by feeble beats, which, though not with complete regularity, increase in force, with shortening intervals, until a maximum is reached, after which the beats decrease in force, and the intervals lengthen, until perfect quiescence is again reached.

A somewhat similar secondary rhythm has been observed in the vertebrate heart as the result of prolonged stimulation of the pneumogastric nerve (compare Luciani, Arbeiten phys. Anstalt, Leipzig, 1873, vii. Jahrgang, p. 113). We have noticed it several times in the snail's heart;

and, as far as we know, the heart had in each instance been treated, in some way or other, by electric currents.

We are totally unable to give any explanation of the phenomenon; and though we have tried in various ways to produce it artificially at will, have always failed to do so.

It suggests the curious inquiry whether, during the quiescent periods, the heart is absolutely at rest, or in reality executing pulsations too small to be visible. We have been unable to detect any indications of such invisible beats, and the marked length of the first and last diastole in each active phase would seem to indicate that the median portion at least of each quiescent phase is occupied by a prolonged diastole of absolute quiescence.

We might further speculate as to whether similar secondary rhythms, marked, not of course by absolute quiescence, but by a more or less pronounced rise and fall of cardiac activity, may not exist under normal circumstances—whether they may not, for instance, be diurnal phases of the heart's own nutrition; and also as to whether the normal pulse-phase may not be made up of small, more rapid, oscillations, bearing the same relation to it as it does to the larger rhythm we are speaking of.

Lastly, if the heart of the snail were a barrel-shaped organ, like that of a Tunicate, with each end equipollent to the other, instead of being a conical mass as it is, with a wholly preponderating auricular end, it is easy to see how such a secondary rhythm as the one we have described would, if started at each end of the tube at different times, produce the well-known alternating action so characteristic of the Tunicate heart.

Experiments on the Hearts of other Mollusks.

The foregoing observations on the heart of the snail were made by us together in the Physiological Laboratory at Cambridge.

During a stay at the Zoological Station at Naples, in the month of November last, Mr. Dew-Smith made several observations on the hearts of *Sepia* and *Aplysia*, with the view to ascertain whether they would behave towards electric currents in the same way as the heart of the snail. We hoped, too, they perhaps might be found more convenient for the purposes of experiment; this hope, however, was not realized.

In the first place the heart of the *Sepia*, though it will beat when removed from the body, can be got to do so with extreme difficulty, and then for a short time only. The observations consequently had to be conducted on the heart *in situ*, and for this reason, as was explained in the case of the snail, were robbed of much of their value.

Moreover the peculiar shape of the ventricle of the *Sepia's* heart, with its two aortæ and its two branchial sinuses, render it much less suitable for the purpose in hand than the more compact and simple ventricle of the snail.

Nevertheless the following results were arrived at:—

Cut into two, so as to divide one aorta and the branchial sinus of one side from the other aorta and the branchial sinus of the other side, each half of the *Sepia's* heart continued to beat rhythmically.

Hence there cannot be a single automatic nervous centre for the ventricle of the *Sepia's* heart.

No nerve could be discovered, the stimulation of which would produce inhibition of the heart after the fashion of the vertebrate pneumogastric.

The interrupted current applied directly to the ventricle did, under favourable circumstances, produce a stoppage of the rhythm, the heart remaining in diastole and resuming its beat on the removal of the current.

Single induction-shocks and the make and break of a constant current appeared to produce the same effects in *Sepia* as in the snail, sometimes causing a contraction and sometimes having as a result a more or less distinct inhibition.

During the passage of a constant current exactly the same phenomena were apparent as in the case of the snail, the beats always starting from the kathode and travelling towards the anode. This effect could, in fact, be seen even more distinctly in the case of *Sepia* than of the snail. By placing the electrodes in a proper position the course of the pulsation along the ventricle might be at pleasure reversed.

In general we may, we believe, safely conclude that what we have found to be the case with the snail's heart, holds good also, in fundamental points, for the heart of *Sepia*.

It was hoped that the large specimens of *Aplysia* which were at hand at the Station would prove of valuable assistance. The difficulty of working with them, owing to the contractions of the body and the unwillingness of the heart to beat out of the body, prevented, however, any satisfactory observations being made with them.

As in *Sepia*, so in *Aplysia*, no nerve could be found, stimulation of which would cause direct inhibition of the heart.

Three specimens of *Salpa* came into Mr. Dew-Smith's hands.

In these two platinum electrodes were thrust through the body, so that their points came to the neighbourhood of the heart, the pulsations of which could be readily observed with the naked eye and counted. The position of the electrodes was such that a current sent through them would pass in large measure lengthwise through the heart.

The effect of the interrupted current was very remarkable. During its passage the heart, which, according to wont, had previously been alternating in direction after every 6 to 15 beats, continued to beat for two minutes *entirely in one* direction at a quickened rate. No inhibition could be produced, even with strong currents.

We quote this single observation because it was the only one that could be made. Bad weather setting in prevented any more *Salpæ* being obtained before Mr. Dew-Smith left for England; otherwise it would

have been extremely interesting to have studied the action of the constant current.

General Considerations.

The beat of the ventricle of the snail's heart may, we venture to think, be regarded as a rhythmic movement of purely protoplasmic nature.

The constituent fibres, if fibres we may venture to call them, are not isolated like the fibres of a vertebrate voluntary muscle, but physiologically continuous; so that any change set up in any part of the ventricle can be propagated over the whole of it in the same way that a contraction-wave set going at any point in a striated fibre is propagated along the whole length of the fibre, or that a contraction-wave is propagated from the point of stimulation along a nerveless ureter. (Compare Engelmann, Pflüger's Archiv, ii. p. 243.)

[The ease with which the entire ventricle of the snail can be completely polarized by the constant current may be regarded as another proof of the physiological continuity of its tissue. The want of conformity between the directions of its fibres and those of the paths naturally taken by the current in passing from one electrode to the other, forbids us to suppose that the effects described above can be the combined result of a number of independently polarized fibres.]

The changes which result in the rhythmic beat take place normally in all parts of the tissue, so that any moiety of the ventricle isolated from the rest is, under satisfactory nutritive conditions, able to execute rhythmic movements. (The isolation may be mechanical, as by section, or physical, as by polarization with the constant current.)

When the ventricle is cut in half, the two halves do not necessarily beat synchronously. Each half has a rhythm of its own, which may or may not be (and in nearly every instance is not) the same in both halves. But the rhythm of each half is, under favourable circumstances, perfect and complete; and the same may be said of still smaller pieces.

Now the normal beat of the entire ventricle is a complicated act. There are in it definite sequences. Certain fibres or certain parts of the tissue begin to contract before others, and certain parts continue to contract after others have ceased to do so. The beat is not a simple contraction-wave passing uniformly from one end to the other, or radiating equally in all directions from one point, but a peculiar movement, having for its object the ejection of fluid from the cavity in the best possible manner, and is hence a coordinated movement.

The aortic half of the ventricle, separated from the auricular half, starts each of its beats quite independently of what is going on in the ventricular half, and *vice versâ*. When the two are physologically continuous, the changes in the one are determined by the changes in the other. In order that a normal beat may be fairly carried through, the auricular half must not start its contraction until the aortic half is ready to

carry it on. Otherwise there would be not a regular beat, but an irregular skirmish of contractions.

We are driven, therefore, to conclude that in the normal condition, where the tissue is physiologically continuous, there are means of communication between all parts; so that the auricular half, for instance, on the one hand, feels, if we may use the phrase, what is going on in the aortic half, and, on the other, exerts an influence on those changes in order to accommodate them to the changes taking place in itself, and *vice versâ.*

We believe that all our knowledge of protoplasm (and we might point especially to the harmonious working of groups of cilia and ciliated cells) favours the idea of the existence of such a *consensus* among the several parts of any mass of protoplasm which acts together as a whole. Nor ought there really to be any difficulty in supposing such communications to be effected by molecular movements in the undifferentiated protoplasm; for there must be in protoplasm many kinds of currents and internal motions for which we have at present no names. We might further urge that there must be in undifferentiated protoplasm the *rudiments* of all the fundamental functions present in the differentiated structures of higher animals. Thus the process by which the condition of the aortic region of the snail's ventricle is communicated to the auricular region seems to be the rudiment of the *muscular sense.*

Adopting some such view as this, we are naturally led to what we venture to think is a more satisfactory interpretation of the part played by ganglia in rhythmic and automatic movements than the one commonly adopted.

The prevalent conception, based on the view put forward by Sir James Paget in his well-known Croonian lecture, if we understand it aright, is somewhat as follows:—Taking, for instance, the automatic ganglia of the heart, motor impulses are rhythmically generated in the ganglionic nerve-cells, as the result of the nutrition of these nerve-cells themselves, influenced, according to circumstances, by afferent impulses starting from sentient surfaces internal or external of the heart itself, or brought from afar by the pneumogastric or other nerves.

These motor impulses reaching the muscular fibres call forth a beat, the contractile elements being, so to speak, mere passive instruments of the nerve-cells, their condition affecting the beat only so far as its force is concerned, inasmuch as, with the same stimulus, the contraction will be greater in a more irritable, and smaller in a less irritable fibre.

Now every one must have felt a difficulty in supposing that the nutrition of a few minute nerve-cells should be all in all, while that of the large mass of the continually and rapidly changing muscular tissue should go for nothing in determining what is at least quite as important as the force of each contraction, viz. the rate of rhythm. Moreover such a view puts, to a certain extent, asunder what nature has evidently, by intricate

ties, joined together—the force and character of the systole and the length of the diastole.

We would suggest that the ganglia are not automatic in function in the above sense, but simple coordinators. The contractile tissue of the frog's ventricle, arranged in bundles isolated by connective tissue, is not physiologically continuous, though each bundle formed by the opposition of branched sheathless muscular fibres may be continuous throughout itself; and the consensus we spoke of just now cannot be effected by molecular communications. Hence the existence of differentiated organs, in the form of nerves and ganglia, by means of which indications of the condition of the isolated constituents of the ventricle (and, we may add, of the isolated auricles and ventricle) are carried, *as items of a muscular sense*, to a central organ, and thus the state of each part is made common to all. In this central organ the advent and character of each beat is determined as the expression of the nutritive condition, not of the nerve-cells only, but of the contractile elements as well.

If this view of the function of automatic ganglia (which, it will be observed, is simply a modification of Sir J. Paget's conception) be correct, it is easy to understand why we have found no such organs in the physiologically continuous protoplasmic heart of the snail.

It needs no such ganglia for the carrying on of its own rhythmic beat; nor does it need them to place it *en rapport* with the rest of the body of the animal. The movements of the body determine the quantity of blood flowing to the heart; the force and rapidity of the heart's beat is in direct ratio to the quantity of blood distending its cavities; and thus a harmony is established between the movements of the body and the circulation quite sufficient for the purposes of the snail's life, without the intervention of that nervous regulative mechanism supplied to the vertebrate heart by the various cardiac ganglia, the pneumogastric, and other nerves.

Résumé.

We have seen that, in the region of the kathode, a condition which we have compared with the kathelectrotonus of nerves is set up. This condition we will not at present venture to characterize more closely than to say that it is favourable to the production of rhythmic beats. At the anode precisely the opposite effect takes place.

We have shown that both conditions require some time for their complete establishment, and that when established they at once begin to decline. Thus they speedily reach a maximum and more gradually subside.

The setting up of kathelectrotonus will, if sufficiently intense and the maximum be fairly reached, give rise to a contraction or beat. The giving way of anelectrotonus, though with less ease, will also cause a beat. Both the establishment of anelectrotonus and the disappearance of kathelectrotonus are unfavourable to the production of the rhythmic beat.

Thus the effect produced by a constant current at each pole is mixed, being partly favourable and partly unfavourable to a beat.

The total effect of the passage of a current, whether a contraction be called forth or not, is unfavourable to the rhythmic beat; and when currents too weak or of too short duration to produce a visible contraction are employed, the unfavourable effect alone becomes evident.

When the brief passage of a weak current is followed at a sufficiently short interval by a similar brief passage, the unfavourable effects are, in a measure, added together; and thus, by repetition, a cumulative result is reached.

Hence it is that a single induction-shock, too weak in itself to produce either a contraction or such a prolongation of the diastole as can be satisfactorily measured, will, if repeated rapidly enough, produce an unmistakable inhibition.

How far the observations we have made, and the explanations we have offered, can be regarded as throwing any light on the working of the vertebrate heart and on the general theory of inhibition, must be determined by further inquiries. These we have already commenced, and hope before long to lay some of our results before the Society.

EXPLANATION OF THE DIAGRAMS (pp. 342, 343).

They are all to be read from right to left.

Each figure consists (except in the case of fig. 6) of three lines. Of these, one, generally the upper, indicates the application of the stimulus.

In the case of single induction-shocks, as in figs. 1 & 2, the descent of the line indicates the making, the ascent the breaking of the primary current. Inasmuch as the making-shock was alone employed, the breaking-shock being stopped, the descent in these figures alone indicates stimulation.

In figs. 3–13 the ascent in the line, generally indicated by x, marks the making, and the descent, generally marked y, indicates the breaking of the current.

The middle line (unless otherwise mentioned) indicates the beat of the ventricle.

In figs. 1–8 the ascent, and in figs. 9–13 the descent, marks the commencement of the beat.

In no case does the interval between the ascent and descent in figs. 1–8, or descent and ascent in figs. 9–13, in any way correspond to the length of the ventricular systole.

The marks on the bottom line in each figure indicate seconds.

Fig. 1. Repeated single induction-shocks.

Two shocks thrown in at x causing distinct prolongation of diastole α, then followed a very feeble beat, then two more shocks at y, followed again by prolongation of diastole β. The stimulations were produced by a Du Bois-Reymond's induction-apparatus, worked by one Daniell's cell. The secondary coil at 8°. The making-shock only was used.

Fig. 2. Single induction-shock.

Single induction-shock thrown in at x, after a series of normal beats. The result was distinct inhibition for a time, then followed a feeble beat at α, then another shock was thrown in at y, followed by a pause. At β the heart commenced to beat again, and resisted the influence of the three succeeding shocks thrown in, continuing to beat normally. Making-shock only. One Daniell's cell. Coil at 5°.

Fig. 3. Constant current applied to a spontaneously beating ventricle.

The upper line indicates the beats of the ventricle, the middle line the application of the current. The kathode was placed at the aortic end of the ventricle, anode placed at the auricular end of the ventricle. No resistance in the resistance-shunt; consequently only the very smallest fraction of the current could have passed through the ventricle. Current made at x and broken at y.

The beat a started from the aortic end, the beat β from the auricular end.

Fig. 4. The same current, applied to the same heart, under exactly similar circumstances as in fig. 3, except that the break almost instantaneously followed the make. No beat was produced, but only a lengthening of the diastole a.

Fig. 5. Effect of constant current applied to the spontaneously beating heart, the auricle still attached to ventricle. Kathode at auricle, anode at aortic end of ventricle. Resistance introduced into the resistance-shunt *nil*; consequently the very feeblest current was sent through the heart. The current is made at x and broken at y. No appreciable effect during the passage of the current, except a slight quickening of the beat. The breaking of the current is marked by the prolonged diastole at a.

Fig. 6. Example of the "secondary rhythm" of the ventricle of the snail's heart.

Shows the secondary rhythm spoken of on page 334. The last of a series of 9 beats is shown at a. Then follows a pause (β) for 18 seconds; then the beats recommence at γ, going on 9 times to δ; then again follows the pause ϵ, succeeded by another series of 9 beats, commencing at ζ.

Fig. 7. Inhibition of ventricle by constant current thrown in during an active period of the secondary rhythm shown in fig. 6.

Auricle still attached to ventricle, and ·1 ohm introduced into the resistance-shunt. The anode at the auricle, the kathode at the aortic end of the ventricle. An instantaneous make and break, whether thrown in during the period of rest or during the period of activity, produced no effect. a is the first of the series of beats succeeding a pause. On the making of the current at x the beats cease, but reappear at β upon the breaking of the current at y.

N.B. The point of the lever marking the ventricular beats on the upper line was not placed exactly over the point of the lever marking on the middle line the application of the current, consequently the beat β seems to precede the breaking of the current at y. In reality it succeeded it by a very short interval.

Fig. 8. Pulsation brought on by constant current thrown in during the resting-period of secondary rhythm.

Heart and strength of current same as in fig. 7. Kathode at auricle, anode at aortic end of ventricle. The resting-period or pause is seen commencing at a, but is broken by the beat β, induced by the constant current, which is made at x; the beats continue, ceasing at γ when the current is broken at y.

Fig. 9. Constant current applied to the heart at rest.

Auricle still attached to the ventricle. Anode at the auricle, kathode at the aortic end of the ventricle. The resistance in the resistance-shunt amounted to 1 ohm. Otherwise the arrangements were the same as in fig. 12 &c. The beat a was very faint, starting from the aortic end of the ventricle. The beats β, γ started from the auricular end of the ventricle. This was an exceptional case, in which a beat (β) appeared before the breaking of the current.

Fig. 10. Constant current.

Applied to heart at rest. Kathode at auricle, anode at aortic end of ventricle.

The top line indicates beat of ventricle; the middle line indicates the application of the current, made at x and broken at y. A single Daniell's cell was used. In the resistance-shunt the resistance was 1 ohm; consequently a small fraction only of the total current passed through the heart.

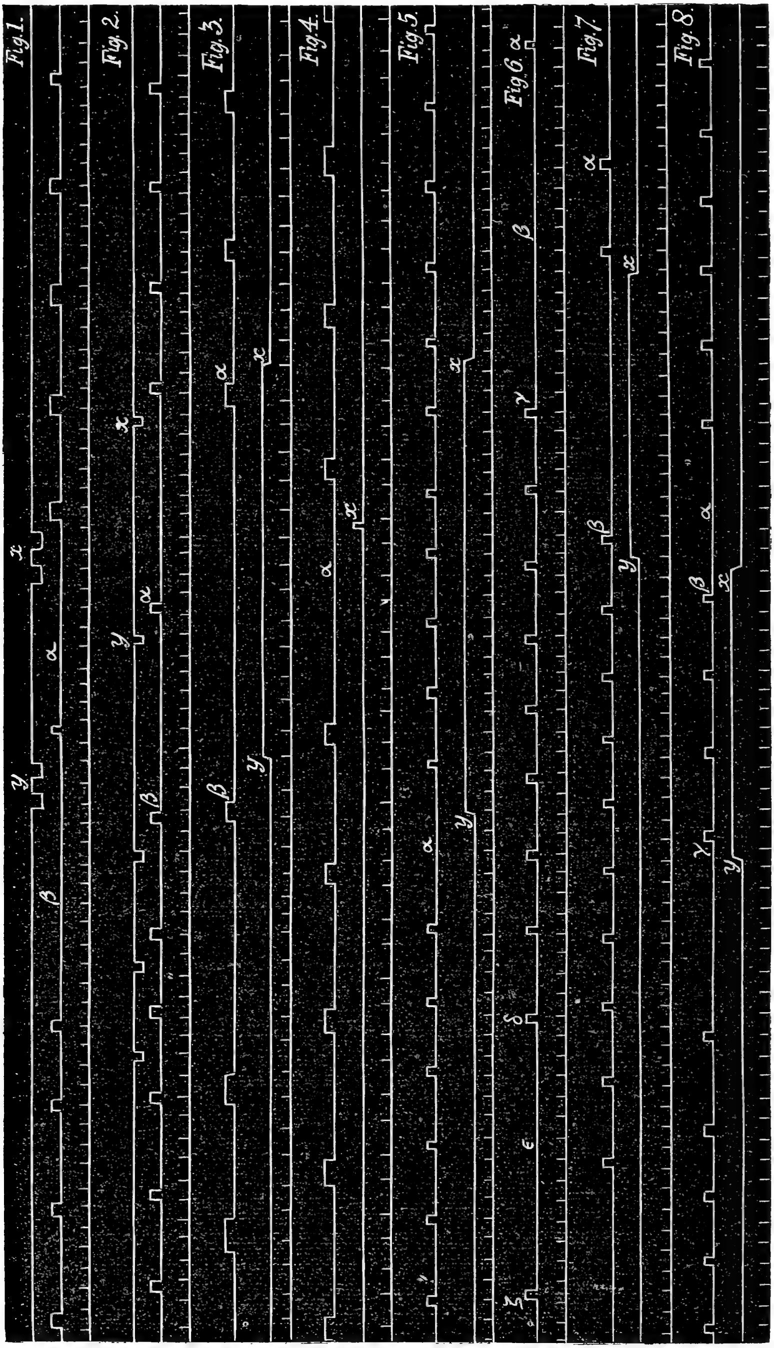
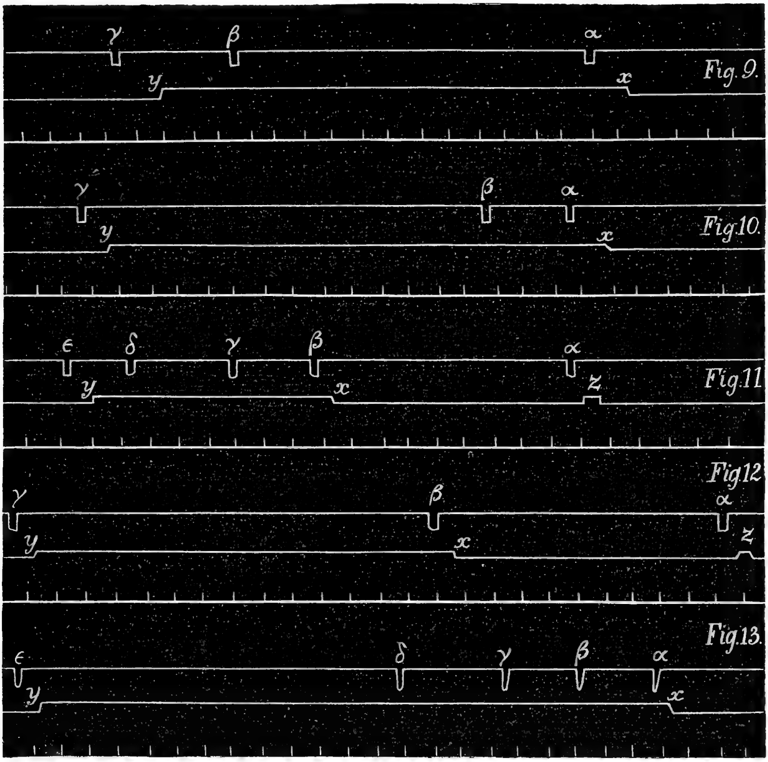


Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.
Fig. 5.
Fig. 6.
Fig. 7.
Fig. 8.



The beats α, β commenced at the auricular end of the ventricle; the beat γ, due to disappearance of anelectrotonus, commenced at the aortic end of the ventricle.

Fig. 11. Same arrangement as fig. 10. Resistance in the resistance-shunt 100 ohms.

The beat at α, due to the instantaneous make and break at z, was normal in character, *i. e.* started from the auricular end of the ventricle. The beats β, γ, δ also started from the auricular end. The beat ϵ started from the aortic end of the ventricle.

Fig. 12. Constant current applied to the heart at rest.

Auricle still attached to ventricle. Anode placed at auricle, kathode at aortic end of ventricle. The whole of the current from a single Daniell's cell was sent through the heart. Otherwise the same arrangement as in figs. 10, 12, & 13.

The beat α, due to the instantaneous make and break at z, was faint, starting from the aortic end of the ventricle. β was a stronger beat, starting also from the aortic end of the ventricle. y was a strong beat, starting from the auricular end of the ventricle.

Fig. 13. Same arrangement as figs. 10 & 11.

The whole of the current from a single Daniell's cell was thrown in. The beats α, β, γ, δ started from the auricular end, and ϵ from the aortic end of the ventricle.

II. "On the Absorption-Spectra of Metals volatilized by the Oxyhydrogen Flame." By J. NORMAN LOCKYER, F.R.S., and W. CHANDLER ROBERTS, Chemist of the Mint. Received February 4, 1875.

The researches which have recently been published on the absorption-spectra of various metals, first by Roscoe and Schuster and subsequently by one of us*, establish beyond all question the facts that—

I. In addition to the well-known line-spectra, channelled-space spectra are produced by the vapours of certain metals; and,

II. Such spectra are produced by vapours which are competent to give at other times, not only line-spectra, but continuous spectra in the blue, or blue and red.

As the temperature employed for the volatilization of the metals in the experiments to which we have referred did not exceed bright redness, or that at which cast iron readily melts, the range of metals examined was necessarily limited. We have therefore considered it desirable to extend these observations to the less fusible metals, as well as to ascertain whether the spectra of those which were volatilized at the lower temperature would be modified by the application of a greater degree of heat. For this purpose we have employed the flame of an oxyhydrogen blowpipe. This instrument, devised by Sainte-Claire Deville and Debray†, renders it possible to attain high temperatures with great facility, and Stas has already employed their method in the distillation of silver‡. The lime still arranged by him has been modified in that about to be described, in order that the metallic vapour might be conducted into a lime tube or tunnel heated to whiteness, so placed that a beam from an electric lamp could readily traverse it.

Description of the Apparatus and method of Manipulation.

The apparatus employed is shown in the figure, in which A is the block of lime § divided horizontally by a plane through the axis of the tube (B B′), this tube being 16 centims. long and 30 millims. diameter. The receptacle (C) communicates with the centre of B B′, and is open at the upper surface of the lime block, in order to admit of the introduction of the oxyhydrogen blowpipe (D), which is provided with a thick nozzle of platinum 20 millims. in diameter. The ends of the tunnel in the lime were closed by glass plates held on by a suitable clip. Small

* Proc. Roy. Soc. vol. xxii. pp. 362 and 371 respectively.

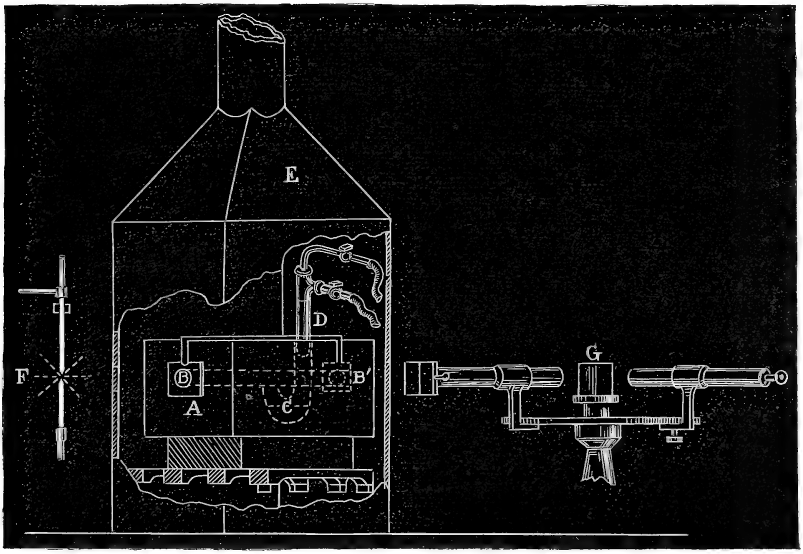
† Ann. de Chimie et de Physique, tom. lvi. p. 413.

‡ Stas, 'Sur les lois des proportions Chimiques,' p. 56.

§ We are indebted to the well-known metallurgist, Mr. J. S. Sellon, of the firm of Johnson and Matthey, for a pure variety of limestone from which the blocks were prepared, and it answered its purpose admirably.

lateral orifices were cut in the lime for the insertion of tobacco-pipe stems, through which a stream of hydrogen could be passed into the tube and receptacle.

An electric lamp (F), in connexion with a 30-cell Bunsen's battery,



was placed opposite one end of the tube, and a spectroscope (G) opposite the other. This last instrument was by Desaga, of Heidelberg, and its single prism, the angle of which was 60°, was capable of distinctly separating the D lines, at the same time that it enabled us to see the whole spectrum in a single field of view, an essential point in such inquiries. The magnifying-power of the telescope was 7·5 linear.

Some preliminary experiments indicated the advisability of increasing the length of the column of vapour. To effect this, a tube 30 centims. long was made in a fresh block of lime, the cavity being arranged as before; in each end a short accurately fitting iron tube, luted with a mixture of graphite and fireclay, was inserted; and the total length of the column thus became 60 centims.

The lime block (C) with its fittings was then placed in the charcoal-furnace (E), by means of which the whole could be raised to a high temperature. As soon as the block was heated to bright redness, the metal, the vapour of which was to be examined, was introduced into the cavity (C), and the flame of the oxyhydrogen blowpipe (D) was allowed to play on its upper surface, care being taken to employ an excess of hydrogen. In almost every case the metal experimented on was rapidly volatilized (the exceptions being gold and palladium). The central portion of the

lime block was raised to a white heat by the action of the blowpipe. As the glass plates rapidly became clouded by the condensation of the metallic vapours, it was necessary to adopt an arrangement by which they could be easily replaced. We may state that, among the precautions which we adopted in order to assure ourselves that oxides were not present to disturb the accuracy of the results, one of the glass plates was removed at the conclusion of each experiment, and the presence of an excess of hydrogen conclusively proved by igniting it at the open end.

We were enabled at any time, by modifying the conditions of the gas-supply, to introduce the spectrum of the oxyhydrogen flame. It may further be stated that, with few exceptions, the metals were previously melted in a stream of hydrogen and enclosed, until experimented on, in sealed glass tubes. We ascertained that the effect of oxides, and of the metallic rain due to condensation, was to produce a general absorption obviously different from the special effects of absorption which we now proceed to record.

Details of the Experiments.

Silver.

Fifty grammes of pure metal were placed in the cavity (C), and this amount produced a continuous supply of vapour for about 10 minutes.

With the smaller thickness given by the first lime block, and with a less powerful blast, the spectrum of silver consisted of an absorption in the blue which at times extended almost to the green.

With the elongated tube and a stronger blast an exquisite channelled-space absorption was observed, the channels being far enough apart to render them very conspicuous in the field of view; at the same time there was continuous absorption in the blue. It was specially observed that there was no absorption in the red.

It may be interesting to note that the vapour of silver when condensed into fine particles, escaping into an atmosphere of hydrogen, is blue by reflected light.

Copper.

With the greatest thickness only a continuous absorption in the blue could be obtained.

Sodium.

Only the *dark* D line was observed, no traces of channelled-space absorption being visible.

Calcium.

We operated upon a small piece of metal prepared by the late Dr. Matthiessen, but no result was obtained.

Aluminium.

When the temperature was so high that the spectrum of the flame was visible, an absorption was suspected in the violet; and the appearance did not change on one glass end being removed.

Zinc.

Many experiments were made on this metal; but there are several points connected with it which require further investigation, and we therefore reserve our remarks on the spectrum of zinc for a future occasion.

Cadmium.

Under both conditions of thickness the vapour of cadmium gave, in the blue only, an absorption which was very decided; an absorption in the red was also noticed which had not been observed in previous experiments when a low temperature was employed.

Manganese.

A small quantity of this metal was prepared with great care by Mr. Bayly, one of the assistant assayers, and it gave a distinct absorption in the red and blue, with evidences of a channelled-space spectrum. In a repetition of the experiment a more distinct channelled-space spectrum was observed.

Iron.

The metal employed had been obtained by electro-deposition in the manner suggested by Mr. Jacobi. Its vapour gave a slight continuous absorption in the blue.

Cobalt

also gave a slight continuous absorption in the blue, but less than in the case of iron.

Nickel.

This metal behaved in the same manner as cobalt, the absorption being about equal in amount.

Chromium.

The amount of metal volatilized was very small, but a fine channelled-space spectrum was observed.

Tin.

This metal caused a considerable absorption in the blue, but less in the red, no traces of a channelled-space spectrum being visible.

Antimony.

In results already published it is stated that at the low temperature antimony gives a channelled-space spectrum. In the present experi-

ments we observed merely absorption in the blue; and this is the only case in which the effects at a high temperature were inferior to those at a low temperature. As the purity of the metal first employed may be doubted, little reliance can be placed on these exceptional results.

Bismuth.

With the greatest thickness the absorption of bismuth is strikingly similar to that of iodine at a dull red heat. We have first a bank of continuous absorption in the blue with a sharp boundary on the less refrangible side, and then a channelled-space absorption throughout the entire green part of the spectrum reaching to D.

Lead.

This metal at first caused an absorption at both ends of the spectrum; shortly afterwards the whole spectrum was extinguished. As this is a readily oxidizable metal, special care was taken to prove that a large excess of hydrogen was present.

Thallium.

We are indebted to Mr. Crookes for a generous supply of this metal. The characteristic green line of thallium was observed *bright*, the light of the arc not being reversed; and it may be interesting to note that the vapour of this metal was incandescent five minutes after the withdrawal of the flame.

Gold.

A distinct absorption in the blue and red was observed, but there were certainly no traces of a channelled-space spectrum. The spectral lines due to the oxyhydrogen flame were very conspicuous. It may be noted that the amount of gold volatilized was only 0·01 oz.; but this quantity of metal was sufficient to produce an abundant supply of vapour.

Palladium.

This metal caused a distinct absorption in the blue, but no effect was noticed at the red end of the spectrum. There was no channelled-space spectrum, and the lines caused by the oxyhydrogen flame were barely visible.

Selenium.

With the greatest thickness employed a channelled-space spectrum was given by selenium.

Iodine.

It will be remembered that, according to the results already published by one of us, iodine vapour exhibits, at a low temperature, a channelled-space spectrum, and a bank of absorption in the violet. These later experiments showed that, at the more elevated temperature, this bank

was broken up and disappeared, leaving a continuous channelled-space spectrum.

These experiments, made at the Royal Mint, were often prolonged for many hours consecutively. They involved much furnace-work of a peculiarly trying nature; and we have much pleasure in acknowledging the assistance we received from Mr. Edward Rigg, one of the assistant assayers, who conducted many of the tedious manipulations with great skill and patience. We should also mention that the care exercised by Joseph Groves, senior fireman, in the preparation of the furnace and the lime-moulds, contributed in no small measure to the success of the experiments.

It appears to us that these experiments, conducted at the high temperature of the oxyhydrogen flame, go far to support the conclusions which were drawn from the experiments at a lower temperature. First, in passing from the liquid to the most perfect gaseous state, vapours are composed of molecules of different orders of complexity; and second, this complexity is diminished by the dissociating action of heat, each molecular simplification being marked by a distinctive spectrum. There is also an intimate connexion between the facility with which the final stage is reached, the group to which the element belongs, and the place which it occupies in the solar atmosphere.

III. "On the Liquation, Fusibility, and Density of certain Alloys of Silver and Copper." By W. Chandler Roberts, Chemist of the Mint. Communicated by Dr. Percy, F.R.S. Received March 11, 1875.

(Abstract.)

The author states that the most remarkable physical property of silver-copper alloys is a molecular mobility, in virtue of which certain combinations of the constituents of a molten alloy become segregated from the mass, the homogeneous character of which is thereby destroyed. These irregularities of composition have long been known, and reference is made to them in the works of Lazarus Erckern (1650) and of Jars (1774). A very complete memoir was published in 1852 by Levol, who did much towards ascertaining the nature and defining the limits of this molecular mobility. He discovered the important fact that an alloy containing 71·89 per cent. of silver is uniform in composition. Its chemical formula ($Ag_3\,Cu_2$) and peculiar structure led him to conclude that all other alloys are mixtures of this, with excess of either metal.

The electric conductivity of these alloys was studied in 1860 by Matthiessen, who doubted the accuracy of Levol's theory, and viewed them as "mechanical mixtures of allotropic modifications of the two metals in each other."

The author then described the experiments he made with a view to determine the melting-points of a series of these alloys. He adopted Deville's determination of the boiling-point of zinc (1040° C.) as the basis of the inquiry, and ascertained, by the method of mixtures, the mean specific heat of a mass of wrought iron between 0° C. and the melting-point of silver, which, as Becquerel showed, is the same as the boiling-point of zinc.

The mean of three experiments, which were closely in accordance, gave 0·15693 as the specific heat; and it should be pointed out that this number includes and neutralizes several errors which would affect the accuracy of the subsequent determinations.

The melting-points of several alloys were then determined by plunging an iron cylinder into them and transferring the iron to a calorimeter. These melting-points varied from 840° C. to 1330° C., or through a range of 490° C. The alloys which occupy the lowest portion of the curve contain from 60 to 70 per cent. of silver. The results are interesting, as they show that the curves of fusibility and electric conductivity are very similar.

The author states that, in studying the phenomena of liquation, the alloys were cast in red-hot moulds of firebrick in which the metal (about 50 oz.) could be slowly and uniformly cooled. The results showed that the homogeneity of Levol's alloy is slightly disturbed by this method of casting; and, on the other hand, that alloys which contain more than 71·89 per cent. of silver hardly show signs of rearrangement when the solidification is gradually effected. Two alloys were examined, which contained 63 and 33·3 per cent. of silver respectively. Both were found to be far from homogeneous. In the case of the former the arrangement was influenced by gravity, the base of the casting being rich in silver.

The density of pure silver and of Levol's homogeneous alloy while in the fluid state were then determined by the method described by Mr. Robert Mallet*, the metals being cast in conical vessels of wrought iron. The results obtained were as follows:—

	Density fluid.	Density solid.
Pure silver..................	9·4612	10·57
Levol's alloy	9·0554	9·9045

In the case of silver, the mean linear expansion deduced from this change of density is ·00003721 per 1° C., which is nearly double the coefficient at temperatures below 100° C.

The Society then adjourned over the Easter Recess, to Thursday, April 8.

* *Vide suprà*, p. 209.

April 8, 1875.

JOSEPH DALTON HOOKER, C.B., President, in the Chair.

Pursuant to notice, the Right Hon. the Earl of Carnarvon and the Right Hon. William Edward Forster were balloted for and elected Fellows of the Society.

Pursuant to notice given at the last Meeting, Pierre J. van Beneden of Louvain, Joseph Louis François Bertrand of Paris, Alfred Louis Olivier Des Cloizeaux of Paris, Hippolyte Louis Fizeau of Paris, Elias Magnus Fries of Upsal, Jules Janssen of Paris, August Kekulé of Bonn, Gustav Robert Kirchhoff of Berlin, and C. Ludwig of Leipzig were balloted for and elected Foreign Members of the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "First Report of the Naturalist attached to the Transit-of-Venus Expedition to Kerguelen's Island, December 1874." By the Rev. A. E. Eaton. Communicated by the President. Received March 15, 1875.

To the Secretary of the Royal Society.

Royal Sound, Kerguelen's Island,
31st December, 1874.

Dear Sir,—It is difficult, owing to the inexactness of the charts, to inform you of the positions of the Astronomical Stations in whose neighbourhood I have been able to work in this island. The German Station is in Betsy Cove, the American at Molloy Point, Royal Sound. The English Stations also are in this Sound, the second being situated about three miles N. by W. of Swain's Haulover. The first English Station is between these last two on the mainland, six or seven miles N.W. of Three-Island Harbour, in what will be called Observatory Bay. Two days before the Transit of Venus, a party under Lieut. Goodridge, R.N., was detached from the first English Station to observe the transit from a position which he selected near the base of Thumb Peak. I have not yet been able to visit Betsy Cove.

Observatory Bay is one of the minor inlets of a peninsula comprised between two narrow arms of the sea. One of these runs up from the sound, along the western flank of the hills adjacent to Mount Crozier, several miles, and terminates at a distance of three or four hours to the north of us, and about four miles from the inlet near Vulcan Cove. The other arm, opening nine or ten miles away to the southward, proceeds

in a north-easterly direction to within three or four miles of the former, and no great distance from Foundry Branch.

Besides the inlets of the sea, numerous freshwater lakes present obstacles to inland travelling. Some in this neighbourhood are two or three miles in length, but in general they are not more than a mile long. They are usually shallow, and appear to be uninhabited by fish. The bogs and streams in this vicinity are not impassable, but can be traversed with ease if ordinary care be taken.

The most salient features of the landscape are the basaltic hills, with irregular terraces of rock on their sides, and broken cliffs at their summits. In lieu of grass, their slopes are clothed with banks and boulder-like clumps of *Azorella selago*, excepting where rich damp loam affords a soil suitable for the *Acæna* and the *Pringlea*. Here and there a fern (*Lomaria*) and grass (*Festuca*) grow in the interspaces of the other plants.

The climate of Royal Sound is far warmer and drier than we were led to expect it would be. In November the weather was very pleasant; since then it has deteriorated, though the snow has not again covered the ground as it did when we first arrived. Probably the previous accounts of its meteorology were based upon observations taken in parts of the island where bad weather prevails; or it may be that the condition of the country in winter has been presumed to be constant throughout the year. In one respect we were rightly informed; for usually when there is no breeze there is a gale. A calm day is an exceptional event. Meteorological observations are being taken in Observatory Bay on board the 'Volage' and by the sappers on shore.

Corresponding with the unlooked for superiority in climate, a difference is noticeable in the vegetation of this part of the island. Some plants which occur at both extremities of the country display, in Royal Sound, marks of luxuriance. For instance, *Pringlea antiscorbutica*, which is elsewhere apetalous, here, in sheltered places, frequently develops petals—some flowers in the same inflorescence possessing one petal only, others having two, three, or four. And the petals are not always of a pale greenish colour, but occasionally are tinged with purple. Again, *Lomaria alpina*, which is mentioned in the flora as rare in the neighbourhood of Christmas Harbour, is excessively common and very finely grown here. There are also more species of flowering plants and of the higher orders of Cryptogamia here than were found by the Antarctic Expedition at the north of the island; but there are fewer species of Mosses, Lichens, and Algæ. Their paucity, in comparison with those of the other district, is probably due to the nature of the rocks on land, and to the seclusion of the bay from the open sea. The additions to the flora are for the most part Falkland-Islands species.

In speaking of the climate, it may be mentioned that the plants of Kerguelen's Island are not (as was supposed) in flower throughout the year;

but, probably, some of them do not cease flowering until late in the winter. When we first arrived in Royal Sound the ground was covered with snow, and scarcely any thing had begun to come out. The *Pringlea* was far advanced in bud, barely commencing to blossom. The *Acæna* was just beginning to burst into leaf. About the first week in November, *Festuca Cookii* came out, and, a few days later, *Azorella selago.* The young fronds of the ferns were just about to unroll. In the third week of the same month, *Montia fontana* and *Acæna affinis* were in flower in a sheltered spot, and *Leptinella plumosa* was first found in blossom. *Galium antarcticum* appeared about the same date. A week later, *Ranunculus hydrophilus* and a *Festuca* (*purpurascens*?) were out, and *Lycopodium clavatum* was sprouting. By the middle of the month, *Trioida* and *Lyallia kerguelensis* and also *Ranunculus crassipes* were in flower; the *Pringlea* was everywhere past flowering (excepting upon the mountains), and *Aira antarctica* began to shoot forth its panicles. Before the end of the month a *Carex* came out; but *Bulliarda* and other plants delayed still.

A few species of Mammals have been introduced into the island. Mice (evidently *Mus musculus*, L.) are common along the coast, and have been found by us in various places. The Rabbits, transported by order of the Admiralty, from the convict settlement in Table Bay have been landed by H.M.S. 'Volage' in Royal Sound. They share with the birds holes of the Petrels, and are (it is almost superfluous to mention) propagating freely. Their favourite food is the *Acæna*; but they occasionally eat *Pringlea*-leaves and gnaw away the green surface of *Azorella.* In the Crozettes, whose climate and flora are said to resemble those of this island, rabbits have become extremely abundant, and so rank and coarse that the sealers will not eat them. Goats are increasing in numbers on the leeward side of the mainland.

Whales and Porpoises occasionally enter the Sound. Old skulls of the latter, wanting the lower jaw, are cast up here and there on the beaches.

Up to the present time, I have captured only two species of Seals—a female Sea-Leopard and two males of a Platyrhine Seal. The other kinds frequent the more open parts of the coast and islands.

Twenty-two species of birds at the fewest, perhaps twenty-three, frequent Royal Sound, viz. a *Chionis*, a Cormorant, a Teal, a Tern, a Gull, a Skua, eleven (perhaps twelve) Petrels, two Albatrosses, and three (perhaps four) Penguins. Of these I have procured eggs of the first six; also of six Petrels, one Albatross, and two Penguins. The *Thalassidromæ* are preparing for laying.

Fish are rather scarce in Observatory Bay. Only three species have hitherto occurred to us, two of which are common under stones at low water. The remains of a *Raia* have also been picked up on one of the islands by an officer of the 'Volage;' but hardly sufficient is left to enable the species to be determined. It is allied to *R. clavata* and *R. radiata.*

The entomology of the island is very interesting. Most of the larger insects seem to be incapable of flight. I have found representatives of the orders Lepidoptera, Diptera, Coleoptera, and Colembola.

The Lepidoptera comprise a species of the *Noctuina* (as I suppose) and one of the *Tineina*. Of the first I have not yet reared the imago; the larva is a moss eater and subterranean: the adult is probably as large as an *Agrotis* of medium size. The species of *Tineina* is probably one of the *Gelechidæ*, judging from the form of the palpi. Its larva feeds on young shoots of *Festucæ*, and sometimes spins a silken cocoon for the pupa. The imago, of which the sexes are alike, has acute and very abbreviated wings, and the posterior pair extremely minute. In repose the antennæ are widely separated and almost divaricate. When the sun shines the adult is active, and, if alarmed, jumps to a distance of two or three inches at a time. During its passage through the air the wings are vibrated.

The Diptera are represented by species of the Tipulidæ and Muscidæ. There are three of the former family. One of them is a small species of the Cecidomyidæ, which is abundant in mossy places, and presents no marked peculiarity. Another seems to be a degraded member of the Tipulidæ. The antennæ have six joints, the palpi two; the wings are ligulate and very minute. It possesses halteres, and the female has the ovipositor enclosed in an exposed sheath. Although it is unable to fly, it lives upon rocks in the sea which are covered at high water, and there it deposits its eggs in tufts of *Enteromorpha*. The third species has full-sized wings; it was caught in the house. The indigenous Muscidæ are very sluggish in their movements, and are incapable of flight. Four species are common about here. One of them is abundant on *Pringlea*, crawling over the leaves. When it is approached it feigns to be dead, and, tucking up its legs, drops down into the axils of the leaves; or, if it happens to be upon a plane surface, one need only look at it closely and it throws itself promptly upon its back and remains motionless until the threatened danger is over, when it gradually ventures to move its limbs and struggle to regain its footing. Its wings are represented by minute gemmules, and it possesses halteres. The ovipositor is extended, its apical joint alone being retracted. The penis is porrected beneath the abdomen, where it fits into a notch at the apex of the penultimate segment. The larva feeds on decaying vegetable matter. Another species occurs on dead birds and animals, as well as beneath stones near the highest tide-mark. It is completely destitute of even the vestiges of wings and halteres. The sexual organs are concealed. It and the preceding species are rather smooth. A third species, slightly hairy, is common amongst tide-refuse and on the adjacent rocks, which are coated with stunted *Enteromorpha*, on which plant, *inter alia*, the larva feeds. It has very small triangular rudiments of wings, slightly emarginate near the apex of the costa, and possesses halteres. The sexual organs are not exposed. The fourth species occurs amongst grass growing

along the shore and also in Shag-rookeries. Its linear and very narrow wings are almost as long as the abdomen. It can jump, but cannot fly. The sexual organs are retracted.

A *Pulex* is parasitic upon *Halidroma*, and one (possibly the same species) on *Diomedea fuliginosa*.

Coleoptera are not uncommon. The larger species seem to have their elytra soldered together. There is a small species of the Brachyelytra.

Several species of *Nirmiidæ* have been obtained.

Two *Poduræ* (one black, the other white) are plentiful.

There appear to be few species of Spiders, though individuals are numerous. Penguins and some of the other birds are infested with Ticks. The remaining Arachnida are related to *Cribates*.

The Crustacea, Annelida, Mollusca, and Echinodermata, in this part of the island, have probably been collected by the 'Challenger' more extensively than I have been able to do; therefore I need not particularize further about them than to state that Entomostraca abound in the lakes; an earthworm is common, and a land-snail is very plentiful amongst the rocks on the hills. This last appears to appreciate comparative heat, for specimens obtained in an exposed place, during the frosty weather, were assembled together for warmth under the drip of an icicle.

In Observatory Bay, Cœlenterata are not numerous. One or two species of Actiniidæ on the rocks and *Macrocystis*-roots, and an Ilyanthid in mud, are the only Actinozoa I have met with. The Hydrozoa similarly have afforded only three species—a Corynid, a Campanularian, and a *Sertularella*.

There are several Sponges.

With the exception of *Limosella aquatica*, and perhaps *Agrostis antarctica*, I have obtained all the flowering plants and ferns given in the 'Flora Antarctica' as indigenous to the island. Besides these, *Ranunculus hydrophilus* and another species, a *Carex*, a *Festuca* (probably *F. purpurascens*; but I have no work containing descriptions of the flowering plants), *Polypodium vulgare*, a fern allied to *Polypodium*, and *Cystopteris fragilis* have occurred to me. There is also a plant which appears to belong to the Juncaceæ. *Lycopodium clavatum* and *L. selago* are common about here. None of the Mosses, Hepaticæ, or Lichens have been worked out as yet; but amongst them are one or two species of *Cladonia*, and some examples of *Lecanora paleacea*. Fungi are represented by *Agaricus* (*Psalliota*) *arvensis*, *Coprinus atramentarius*, and a peculiar parasite on *Azorella*, which grows out from the rosettes in the form of a clear jelly, which becomes changed into a firm yellowish substance of indefinite form. There are also some *Sphæriacei* on grass and dead stems of plants. At present few additions have been made to the marine flora. The larger Algæ in Royal Sound are usually not cast upon the shore by the waves, and I have almost been entirely dependent upon grapples thrown from the rocks for specimens of the more delicate forms. *Polysiphonia Sullivanæ*

and *Rhytiphlœa Gomardii* are amongst the novelties. A large number of zoological and botanical specimens have been lost through my inability to attend to them in time without assistance. This has principally affected the number of duplicates; but in one instance it has led to the loss of a species—one of the Petrels, which was the commonest bird about here when we first arrived. Fortunately it is a well-known species.

The 1st of March is announced as the approximate date of our sailing from Kerguelen's Island. Five weeks later I hope to arrive at the Cape and to forward to you such of the specimens collected as require only ordinary care in their transmission. The more fragile things are likely to reach you in better condition if I keep them until my return to England, than they would if they were sent with the others.

I am, dear Sir,

Faithfully yours,

A. E. Eaton.

II. "Experiments to ascertain the Cause of Stratification in Electrical Discharges *in vacuo*." By Warren De La Rue, Hugo W. Müller, and William Spottiswoode. Received February 24, 1875.

Some results obtained in working with a chloride-of-silver battery of 1080 cells in connexion with vacuum-tubes appear to be of sufficient interest to induce us to communicate them to the Society, in anticipation of the more detailed account of an investigation which is now being prosecuted, and which it is intended to continue, shortly, with a battery of 5000 cells, and possibly with a far greater number.

The battery used up till now consists of 1080 cells, each being formed of a glass tube 6 inches (15·23 centims.) long and $\frac{3}{4}$ of an inch (1·9 centim.) internal diameter; these are closed with a vulcanized rubber stopper (cork), perforated eccentrically to permit the insertion of a zinc rod, carefully amalgamated, $\frac{3}{16}$ (0·48 centim.) of an inch in diameter and 4·5 inches (11·43 centims.) long. The other element consists of a flattened silver wire passing by the side of the cork to the bottom of the tube and covered, at the upper part above the chloride of silver and until it passes the stopper, with thin sheet gutta percha for insulation, and to protect it from the action of the sulphur in the vulcanized corks; these wires are $\frac{1}{16}$ of an inch (0·16 centim.) broad and 8 inches (20·32 centims.) long. In the bottom of the tube is placed 225·25 grains (14·59 grms.) chloride of silver in powder; this constitutes the electrolyte: above the chloride of silver is poured a solution of common salt containing 25 grammes chloride of sodium to 1 litre (1752 grains to 1 gallon) of water, to within about 1 inch (2·54 centims.) of the cork. The connexion between adjoining cells is made by passing a short piece of india-rubber tube

over the zinc rod of one cell, and drawing the silver wire of the next cell through it so as to press against the zinc. The closing of the cells by means of a cork prevents the evaporation of water, and not only avoids this serious inconvenience, but also contributes to the effectiveness of the insulation. The tubes are grouped in twenties in a sort of test-tube rack, having four short ebonite feet, and the whole placed in a cabinet 2 ft. 7 in. (78·74 centims.) high, 2 ft. 7 in. wide, and 2 ft. 7 in. deep, the top being covered with ebonite to facilitate working with the apparatus, which is thus placed on it as an insulated table.

The electromotive force of the battery, as compared with a Daniell's (gravity) battery, was found to be as 1·03 to 1*, its internal resistance 70 ohms per cell, and it evolved 0·214 cub. centim. (0·0131 cub. inches) mixed gas per minute when passed through a mixture of 1 volume of sulphuric acid and 8 volumes of water in a voltameter having a resistance of 11 ohms. The striking-distance of 1080 elements between copper wire terminals, one turned to a point, the other to a flat surface, in air is $\frac{1}{263}$ inch (0·096 millim.) to $\frac{1}{250}$ inch (0·1 millim.). The greatest distance through which the battery-current would pass continuously *in vacuo* was 12 inches (30·48 centims.) between the terminals in a carbonic acid residual vacuum. This battery has been working since the early part of November 1874, with, practically, a constant electromotive force.

Besides 2000 more cells like those just described, we are putting together 2000 cells, with the chloride of silver in the form of rods, which are cast on the flattened silver wires, as in a battery described by De La Rue and Müller †, but in other respects similar to the battery above described, the glass tubes being, however, somewhat larger in diameter; the rods of chloride of silver are enclosed in tubes open at the top and bottom, and formed of vegetable parchment, the object of these vegetable-parchment cases being to prevent contact between the zinc and chloride-of-silver rods. The internal resistance of batteries so constructed is only from 2 to 3 ohms per cell, according to the distance of the zinc and chloride-of-silver rods, and they evolve from 3 to 4·5 cub. centims. (0·18 to 0·27 cub. inch) per minute, in a voltameter having a resistance of 11 ohms. Their action is remarkably constant.

For the experiments detailed below, vacuum-tubes were generally used of about $1\frac{1}{2}$ to 2 inches (3·8 to 5 centims.) in diameter, and from 6 to 8 inches (15·24 to 20·32 centims.) long; also prolate spheroidal vessels 6 inches by 3 inches (15·24 by 7·62 centims.). The terminals are of various forms, and from 4 inches to 6 inches (10·16 to 15·24 centims.) apart, and made of aluminium and occasionally of magnesium and of palladium,

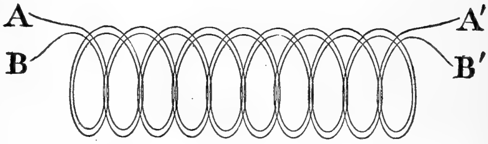
* Compared with a Daniell's battery, in which the zinc is immersed in dilute sulphuric acid in a porous cell, its electromotive force is about 3 per cent. less than the Daniell.

† Journal of the Chem. Soc., 2nd series, vol. vi. p. 488; Comptes Rendus, 1868, p. 794.

the latter showing some curious phenomena with a hydrogen residual vacuum, which will be described in a future paper. A tube which has given the most striking results is 8 inches (20·32 centims.) long, and has a series of six aluminium rings varying in diameter from $\frac{3}{8}$ of an inch to about $1\frac{1}{4}$ of an inch (0·95 to 3·17 centims.), the thickness of the wire being about $\frac{1}{16}$ (0·16 centim.) of an inch; the rings are a little more than 1 inch (2·54 centims.) apart; and connecting wires of platinum pass through the tube from each ring and permit of the length and other conditions of the discharge being varied.

At times the terminals of the battery were placed in connexion with accumulators of different kinds—for instance, two spheres of 18 inches (45·72 centims.) in diameter, presenting each a superficies of 7·07 square feet (65·68 square decims.), and cylinders of paper covered with tinfoil, each having a surface of 16 square feet (148·64 square decims.); the globe and cylinders were in all cases carefully insulated. Other accumulators were composed of coils of two copper wires $\frac{1}{16}$ of an inch (0·16 centim.) in diameter, covered with gutta percha, in two folds, $\frac{1}{32}$ of an inch (0·08 centim.) thick. One coil contains two wires, A A′ and B B′ (fig. 1), coiled side by side, each being 174 yards (159 metres) long, another with two wires each 350 yards (320 metres) long; of the latter we have two coils.

Fig. 1.



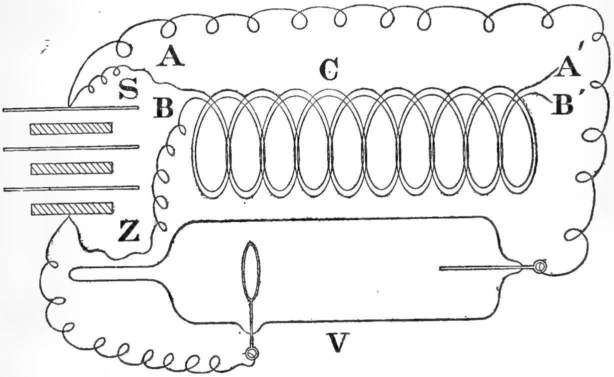
In addition to these accumulators we have several others formed of alternate plates of tinfoil and insulating material, such as paper saturated with paraffine, and also sheets of vulcanite. These are of various capacities and contain from 5 to several hundred square feet. The largest has a capacity of 47·5 microfarads; when it is discharged it gives a very bright short spark, accompanied by a loud snap; the charge deflagrates 8 inches (20·32 centims.) of platinum wire, ·005 inch (0·127 millim.) in diameter, when it is caused to passed through it. Each accumulator gives different results; but for the present we shall confine ourselves to a description of the experiments made with the coil-accumulators.

When the terminals of the battery are connected with the wires of a vacuum-tube which permits of the passage of the current, the wires (especially that connected with the zinc end) become surrounded with a soft nebulous light, in which several concentric layers of different degrees of brilliancy are seen; in most cases there is either no indication of stratification, or only a feeble ill-defined tendency to stratification: the tubes

selected for these experiments were those in which the stratification did not appear at all.

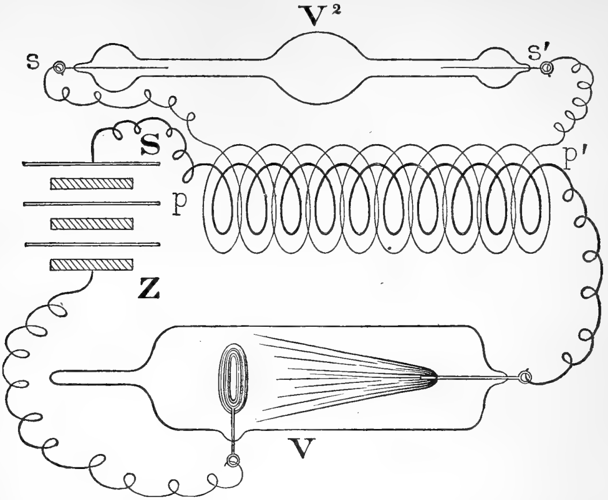
When the battery, already in connexion with the vacuum-tube, was also joined, as in fig. 2, on to one or more coil-condensers (coupled to introduce a greater length of wire) in the following manner, then immediately well-defined stratifications appeared in the vacuum-tube.

Fig. 2.



S Z represents the battery, V the vacuum-tube, C the coil-condenser; one terminal is connected with the end A of the wire A A′, and the other terminal with the end B of the second wire B B′; connexions are also led to the wires of the vacuum-tube. The ends A′ and B′ are left free; and it is clear that the coil forms a sort of Leyden jar when thus used: an interval, however short it may be, must elapse in accumulating a charge which at intervals discharges itself and causes a *greater flow* in the vacuum-tube in addition to that which passes continuously. It may be stated that the capacity of the accumulator has to be carefully adjusted to prevent any cessation of the current, to avoid, in fact, a snapping discharge at distant intervals. The periodic overflows, so to speak, which increase the current from time to time, would seem to have a tendency to cause an interference of the current-waves, and to produce nodes of greater resistance in the medium, as evinced by the stratification which becomes apparent. To the eye no pulsation in the current is apparent; and in order to convince ourselves whether or not there was really any fluctuation in the current when the apparatus was thus coupled up with the battery, we made several experiments, and ultimately hit upon the following arrangement (fig. 3):—

Fig. 3.



The primary wire p p′ of a small induction-coil, both with and without the iron core, was introduced into the circuit as well as the vacuum-tube V; to the secondary wire s s′ of the induction-coil was connected a second vacuum-tube, V^2. Under these circumstances there was no change in the appearance of the discharge in V, in consequence of the introduction of the induction-coil, the terminals being still surrounded by the soft nebulous light before spoken of: no luminosity appeared in the second vacuum-tube V^2 in connexion with the secondary wire of the induction-coil, except on making and breaking the connexion with the battery. At other times there was evidently no fluctuation in the continuous discharge, no periodic increase or diminution of flow, and consequently no induced current in the secondary wire s s′ of the induction-coil.

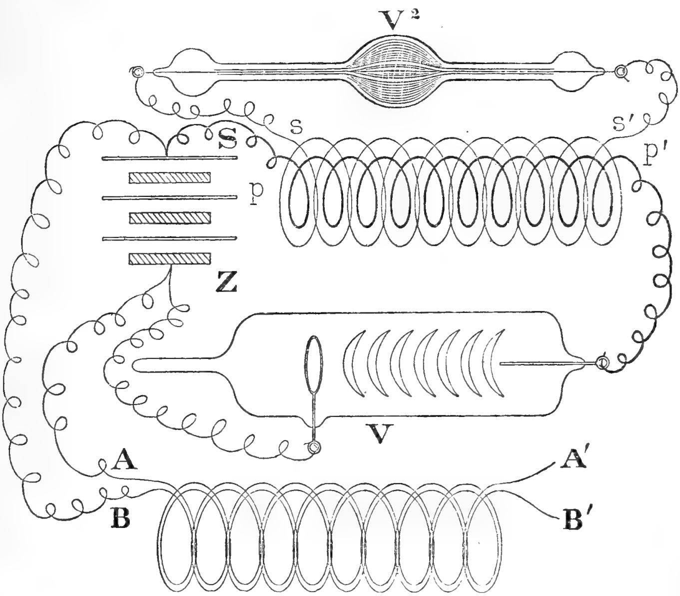
In the second experiment wires were also led from the terminals of the battery (all other things remaining as before) to the coil-accumulator as in fig. 4; then immediately the discharge in V became stratified and the secondary vacuum-tube V^2 lighted up, clearly showing that under these circumstances a fluctuation in the discharge really occurs on the appearance of stratification.

The brilliancy of the discharge in V^2 (the induced current passes through complicated vacuum-tubes through which the primary current cannot pass) depends greatly on the quality and quantity of the discharge in the primary vacuum-tube V. Under some circumstances the secondary discharge is extremely feeble, and the illumination in V^2 barely visible; under others it is very brilliant.

Preparations are being made to render evident induced currents in the

secondary wire of the coil too feeble to produce any illumination. Pending the further development of our investigation, we have ventured to give an account of our progress in elucidating some points in the theory of the vacuum-discharge, without any wish to ascribe to our results more weight than they deserve.

Fig. 4.



Batteries of this description may be had from Messrs. Tisley and Spiller, Brompton Road. Their cost, in large numbers, is about one shilling per cell, exclusive of the charge of chloride of silver, which costs about two shillings per cell. The latter, either in the form of powder or of rods cast upon flattened silver wire, may be obtained from Messrs. Johnson and Matthey, Hatton Garden. When the battery is exhausted the reduced silver may be readily reconverted into chloride, with scarcely any loss.

April 15, 1875.

JOSEPH DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Development of the Teeth of Fishes (Elasmobranchii and Teleostei)." By CHARLES S. TOMES, M.A. Communicated by JOHN TOMES, F.R.S. Received March 1, 1875.

(Abstract.)

It has usually been supposed that the whole process of the development of the teeth in many fish might be taken to represent the earlier stages alone of the process as it is seen in man; this opinion is forcibly expressed by Professor Owen, who, for example, says of the sharks, "Here is represented the first and transitory papillary stage of dental development in mammals; and the simple cartilaginous maxillary plate, with the open groove behind containing the germinal papillæ of the teeth, offers, in the shark, a magnified representation of the earliest condition of the jaws and teeth of the human embryo."

With this opinion, already objected to by Professor Huxley, I cannot concur; still less can I concur with the statement that "in all fish the first step is the simple production of a soft vascular papilla from the free surface of the buccal membrane."

The supposed open groove behind the jaws of cartilaginous fish is in reality closed; that is to say, the epithelium of the jaw passes continuously across on to the thecal fold of mucous membrane which lies behind the jaw and protects the developing teeth; and, if the groove be opened, it is by the tearing through of this epithelium.

Near to the base of the jaw, the mucous membrane is cellular and dentine-organs are formed by its elevation into conical papillæ, without apparent structural change; but, higher up, the dentine-papillæ assume their characteristic forms and structure, and the residuum of the mucous membrane at their bases becomes transformed into a fibrillated tissue.

As the teeth become yet more advanced, the fibrillated tissue at their bases becomes specially arranged with reference to the base of each tooth, so as to form, in some sense, ligaments to bind it firmly in its place.

Just as the mucous membrane of the exposed surface of the jaw is covered by its epithelium, so the dentine-papillæ are covered by theirs; in the latter place, however, it has undergone a special development, which entitles it to the name of an "enamel-organ." As was originally pointed out by Professor Huxley, enamel-organs are but modified epithelium.

The dentine-papillæ are processes arising from a continuous sheet of mucous membrane; the enamel-organs also are continuous with one another, attaining to a special development where they serve as caps to the several dentine-papillæ; the arrangement of these enamel-organs suggests a resemblance to the corresponding structures described by me as existing in the newt and in certain reptiles.

Enamel is present in noteworthy thickness in some Elasmobranchiate fish; it is absent, or almost so, in others; but enamel-organs are present in all. The enamel-organs consist, on the surfaces directed towards the

dentine-papillæ, of a well-marked columnar epithelium (enamel-cells); and, behind this layer, of a sort of finely fibrous tissue with branched cells, not, however, resembling that known as the reticulum in mammalian enamel-organs.

In young specimens, before the continuity of the two structures is interrupted by the presence of a lip, the homological identity of the teeth and the dermal spines is well seen, the one passing into the other in an unbroken series; the teeth, however, even at an early period, attain to a much larger size than the contiguous dermal spines.

Amongst osseous fish, my observations have been principally made upon the perch, pike, eel, haddock, cod, mackerel, and herring.

Allowing for differences of detail, which must necessarily result from the varying configuration of the jaws, &c., the process is identical in all the fish which I have examined, and is similar to that which I have observed in reptiles.

From the oral epithelium there dips down a process, the terminal end of which becomes transformed into an enamel-organ, the contiguous subjacent tissue coincidently becoming developed into a dentine-papilla.

I have seen nothing which could be called a "free papilla;" and it is my conviction that free papillæ at no time exist in any animal; but it is possible that Professor Owen's statement, that "in all fishes the first step is the simple production of a soft vascular papilla from the free surface of the buccal membrane," may have been based upon appearances such as are met with in the haddock, in which fish (in certain situations) the tissues surrounding, and lying over, the forming tooth-sac do become elevated, so that on the surface there is a papilliform eminence; this, however, is quite external to the real dentine-papilla, and is altogether extraneous to the tooth-sac, which does not make up one fourth of its bulk.

The distance from the surface at which the formation of the tooth-sac takes place seems to be variable, differing even in the same fish in different situations.

The enamel-organs of the eel and perch are peculiar, consisting mainly of the layer of "enamel-cells;" over the apex of the tooth these enamel-cells are three times as large as over its sides, the transition from cells of the one size to the other being abrupt, and not gradual.

Their teeth are surmounted by terminal caps of enamel, like those of the newts and salamanders, or those figured by Professor Owen upon the teeth of "*Ganacrodus*," a new genus founded upon this solitary character: enamel is absent from the sides of the teeth, or, if present, is in so thin a layer as to be difficult to detect with certainty.

Thus the one part of the enamel-organ appears to exercise an active function, the remainder to be rudimentary; and the position of the enamel-cells of large size coinciding with the distribution of the enamel, is, so far as it goes, evidence in favour of the hypothesis of the formation of enamel by direct conversion of the cells.

Prof. Huxley first correctly determined the homologies of the enamel-organ and the dentine-papilla, referring the first to the epithelium, the latter to the derm; the follicle, however, where it exists at all, I regard as mainly a secondary development from that region of the derm which formed the base of the dentine-germ.

Observations upon many mammals, reptiles, and fishes lead me to the following general conclusions as to the development of teeth:—

(i.) All tooth-germs whatever consist, in the first instance, of two parts, and two alone—the dentine-papilla and the enamel-organ.

(ii.) The existence of an enamel-organ is wholly independent of the presence or absence of enamel upon the teeth; examples of this have been recorded by Professor Turner and by myself among mammalia, and by myself among reptiles and fishes.

(iii.) Nothing justifies the arbitrary division into "papillary," "follicular," and "eruptive" stages; nor does any open primitive dental groove, or fissure, exist in any animal examined.

(iv.) In all cases, an active ingrowth of a process of the oral epithelium, dipping inwards into solid tissue, is the first thing distinguishable; although the formation of a dentine-papilla, opposite to its deepest extremity, goes on *pari passu* with the development of its cæcal end into an enamel-organ.

(v.) A special capsule, or follicle, to the tooth-germ may or may not be present; when present, it is, in part, a secondary development from the base of the dentine-papilla, in part, a mere condensation of surrounding tissue.

II. "Researches upon the Specific Volumes of Liquids." By T. E. Thorpe. Communicated by Prof. Williamson, For. Sec. R.S. Received March 2, 1875.

(Abstract.)

I. *On the Atomic Value of Phosphorus.*

Hermann Kopp has shown that, as a rule, the specific volume of an element is invariable when in combination. Exceptions to the law occur, however, in the cases of oxygen and sulphur, each of which bodies has two specific volumes dependent upon the manner in which they are held in union. When contained "within the radicle," as in acetyl, C_2H_3O, oxygen has the value 12·2, but when existing "without the radicle," as in alcohol, it has the smaller value, 7·8. Sulphur, when "within the radicle," has the specific volume 28·6; when "without the radicle," it has the specific volume 22·6.

The cause of these variations may be thus stated in the language of modern theory:—When dyad sulphur and oxygen are united to an element by both their affinities, their specific volumes become respectively

28·6 and 12·2; when they are attached by only one combining unit, their specific volumes are 22·6 and 7·8.

Phosphorus is regarded by certain chemists as invariably a triad; others maintain that it is sometimes a triad, at other times a pentad. In the trichloride it is a triad, in the oxychloride and thiochloride it is a pentad. According to this view, the two latter compounds possess the following constitution:—

$$\begin{array}{ccc} & \mathrm{Cl} & \\ & | & \\ \mathrm{Cl}- & \mathrm{P} & =\mathrm{O} \\ & | & \\ & \mathrm{Cl} & \end{array} \qquad\qquad \begin{array}{ccc} & \mathrm{Cl} & \\ & | & \\ \mathrm{Cl}- & \mathrm{P} & =\mathrm{S} \\ & | & \\ & \mathrm{Cl} & \end{array}$$

If, however, phosphorus is invariably trivalent, the oxychloride and thiochloride must possess the formulæ

$$\begin{array}{cl} \mathrm{Cl} & \\ | & \\ \mathrm{P} & -\mathrm{O}-\mathrm{Cl} \\ | & \\ \mathrm{Cl} & \end{array} \qquad\qquad \begin{array}{cl} \mathrm{Cl} & \\ | & \\ \mathrm{P} & -\mathrm{S}-\mathrm{Cl} \\ | & \\ \mathrm{Cl} & \end{array}$$

It is possible to decide between the two modes of representing the constitution of these compounds, if it be granted that the variation in the specific volume of oxygen and sulphur is due to the manner in which these elements are held in union. For, if the phosphorus in the oxychloride and thiochloride be quinquivalent, the oxygen and sulphur must possess the greater of the two values, since both their combining units are united to the phosphorus; if, on the other hand, phosphorus be trivalent, the oxygen and sulphur must possess the smaller of the two values.

The author has determined the specific gravity, boiling-point, and rate of expansion of $P\,Cl_3$, $P\,O\,Cl_3$, and $P\,S\,Cl_3$, in order to ascertain the specific volume of the oxygen and sulphur in the two latter compounds, and consequently the chemical value of the phosphorus; and he finds that the specific volumes of the oxygen and sulphur are almost identical with the values given by Kopp for these elements when "without the radicle." It would therefore appear that the oxychloride and thiochloride must possess the constitution

$$\begin{array}{cl} \mathrm{Cl} & \\ | & \\ \mathrm{P} & -\mathrm{O}-\mathrm{Cl} \\ | & \\ \mathrm{Cl} & \end{array} \qquad\qquad \begin{array}{cl} \mathrm{Cl} & \\ | & \\ \mathrm{P} & -\mathrm{S}-\mathrm{Cl} \\ | & \\ \mathrm{Cl} & \end{array}$$

and that the phosphorus in these bodies is to be regarded as a triad.

The author concludes by discussing Buff's hypothesis, that the specific volume of an element varies with its chemical value; and he shows that, in the case of phosphorus, there are no reasons for the belief that this element has a variable specific volume.

Presents received, March 4, 1875.

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April 8, 1875.

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Engraved Portrait of Sir James Paget, F.R.S., after J. E. Millais, R.A., by T. O. Barlow, R.A. (framed and glazed). T. O. Barlow, R.A.

April 15, 1875.

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April 22, 1875.

JOHN EVANS, Esq., Vice-President, in the Chair.

The Right Hon. Russell Gurney, Q.C., whose certificate had been suspended, as prescribed by the Statutes, was elected a Fellow of the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On the Action of Heat on the Absorption-Spectra and Chemical Constitution of Saline Solutions." By WALTER NOEL HARTLEY, F.C.S., Demonstrator of Chemistry, King's College, London. Communicated by Prof. STOKES, Sec. R.S. Received March 10, 1875.

(Abstract.)

The effects of heat on absorption-spectra were recorded in the preliminary notice of this paper, published in the 'Proceedings of the Royal Society' for 1874 (vol. xxii. p. 241).

The contents of the present communication consist of :—1st, historical notes ; 2nd, method of working ; 3rd, the spectrum-measurements of different solutions ; 4th, conclusions as to the effect of heat on coloured liquids, and the following deductions as to the constitution of salts when dissolved in water :—

I.

When a simple metallic salt is dissolved in water, it is not decomposed in such a way that an oxide and an acid is produced, nor does a compound of the metallic oxide with the acid result.

II.

When a metallic salt is dissolved in water to form a saturated solution, it does not necessarily attain its maximum state of hydration.

III.

When a simple hydrated metallic salt is dissolved in water to form a saturated solution, the crystalline molecule remains chemically intact, except in the case of certain compounds which readily part with their water of crystallization, when dehydration takes place to form a molecule of greater stability ; or, in other words, solution facilitates chemical change in this as in most other cases.

IV.

When a simple salt assumes one or more definite states of hydration at different temperatures below 100° C., the hydrated compounds A and B will be successively produced in the liquid state if a saturated solution of the original salt be heated to 100° C.; or, in other words, the chemical constitution of the liquid is altered so that, as higher temperatures are attained, it becomes a solution of substance A or of substance B, at intermediate temperatures mixtures of these.

V.

The action of heat on the violet hydrated compounds of chromium is not simply a dissociation of water-molecules or of acid from base, but a true decomposition, resulting in the production of a different class of salts with different generic properties.

Many new salts were prepared for this work, and others were examined with greater care than had previously been bestowed on them; from these substances, indeed, the most important part of the results were derived.

II. "On Attraction and Repulsion resulting from Radiation."—Part II. By William Crookes, F.R.S. &c. Received March 20, 1875.

(Abstract.)

This is the second part of a paper which the author sent to the Royal Society in August 1873. The author commences by describing improvements which he has made in the Sprengel pump, and in various accessories which are necessary when working at the highest rarefactions.

Continuing the description of apparatus, the author describes different new forms which enable the phenomena of repulsion by radiation to be observed and illustrated. A bulb 3 inches in diameter is blown at the end of a glass tube 18 inches long. In this bulb a fine glass stem, with a sphere or disk of pith &c. at each end, is suspended by means of a cocoon-fibre. The whole is attached to the Sprengel pump in such a way that it can be perfectly exhausted and then hermetically sealed. Besides pith, the terminals may be made of cork, ivory, metal, or other substance. During exhaustion several precautions have to be taken, which are fully entered into in the paper. To get the greatest delicacy in an apparatus of this kind, there is required large surface with a minimum of weight. An apparatus constructed with the proper precautions is so sensitive to heat, that a touch with the finger on a part of the globe near one extremity of the pith will drive the index round over 90°, whilst it follows a

piece of ice as a needle follows a magnet. With a large bulb, very well exhausted and containing a suspended bar of pith, a somewhat striking effect is produced when a lighted candle is placed about 2 inches from the globe. The pith bar commences to oscillate to and fro, the swing gradually increasing in amplitude until the dead centre is passed over, when several complete revolutions are made. The torsion of the suspending fibre now offers resistance to the revolutions, and the bar commences to turn in the opposite direction. This movement is kept up with great energy and regularity as long as the candle burns.

The author discusses the action of ice, or a cold substance, on the suspended index. Cold being simply negative heat, it is not at first sight obvious how it can produce the opposite effect to heat. The author, however, explains this by the law of exchanges, and shows that attraction by a cold body is really repulsion by radiation falling on the opposite side. According to the same law, it is not difficult to foresee what will be the action of two bodies, each free to move, if they are brought near to one another in space, and if they differ in temperature either from each other or from the limiting walls of the space. The author gives four typical cases, with experiments, which prove his reasoning to be correct.

Experiments are described with the object of ascertaining whether the attraction by heat, which, commencing at the neutral point, increases with the density of the enclosed air, will be continued in the same ratio if the apparatus is filled with air above the atmospheric pressure. This is found to be the case.

Various experiments are described with bulb-apparatus, in which the bulb is surrounded with a shell containing various adiathermous liquids and also with a shell of vacuum. In all cases radiation passed through, producing the normal action of attraction in air and repulsion in a vacuum.

The author next describes a form of apparatus by which measurable results are attainable. It consists of a long glass tube, with a wider piece at the end. In it is suspended a lump of magnesium by a very fine platinum wire, the distance between the point of suspension and the centre of gravity of the magnesium bob being 39·14 inches. Near the magnesium is a platinum spiral, capable of being ignited by a voltaic battery. Observations of the movement of the pendulum are made with a telescope with micrometer eyepiece. With this apparatus a large series of experiments are described, starting from air of normal density, and working at intermediate pressures up to the best attainable vacuum. The results are given in two tables.

With this apparatus it was found that a candle-flame brought within a few inches of the magnesium weight, or its image focused on the weight and alternately obscured and exposed by a piece of card at intervals of one second, will soon set the pendulum in vibration when the vacuum is

very good. A ray of sunlight allowed to fall once on the pendulum will immediately set it swinging.

The form of apparatus is next described which the author has finally adopted, as combining the greatest delicacy with facility of obtaining accurate observations, and therefore of getting quantitative as well as qualitative results. It consists of a glass apparatus in the shape of an inverted T, and containing a horizontal glass beam suspended by a very fine glass thread. At the extremities of the beam are attached the substances to be experimented on, and at the centre of the beam is a small mirror from which a ray of light is reflected on to a graduated scale. The advantage which a glass thread possesses over a cocoon-fibre is that the index always comes accurately back to zero. In order to keep the luminous index at zero, except when experiments are being tried, extreme precautions must be taken to keep all extraneous radiation from acting on the torsion-balance. The whole apparatus is closely packed all round with a layer of cotton-wool about 6 inches thick, and outside this is arranged a double row of Winchester quart bottles filled with water, spaces only being left for the radiation to fall on the balance and for the index ray of light to get to the mirror.

However much the results may vary when the vacuum is imperfect, with an apparatus of this kind they always agree among themselves when the residual gas is reduced to the minimum possible; and it is of no consequence what this residual gas is. Thus, starting with the apparatus full of various vapours and gases, such as air, carbonic acid, water, iodine, hydrogen, ammonia, &c., at the highest rarefaction, there is not found any difference in the results which can be traced to the residual gas. A hydrogen-vacuum appears the same as a water- or an iodine-vacuum.

With this apparatus the effect of exposing a torsion-balance to a continuous radiation is described, and the results are shown graphically. The effect of a short (11·3 seconds) exposure to radiation is next described, and the results are given in the form of a Table.

In another Table is given the results of experiments in which a constant source of radiation was allowed to act upon one end of the torsion-beam at a distance of 140 or 280 millims., various substances being interposed. The sensitiveness of this apparatus to heat-rays appears to be greater than that of an ordinary thermo-multiplier. Thus the obscure heat-rays from copper at 100°, passing through glass, produce a deflection on the scale of 3·25, whilst under the same circumstances no current is detected in the thermo-pile. The following substances are used as screens, and the deflections produced (when the source of radiation is magnesium wire, a standard candle, copper at 400°, and copper at 100°) are tabulated:—

Rock-salt, 20 millims. thick; rock-crystal, 42 millims. thick; dark smoky talc; plate glass of various thicknesses, both white and green; a

glass cell containing 8 millims. of water; a plate of alum 5 millims. thick; calc-spar, 27 millims. thick; ammonio-sulphate of copper, opaque to rays below F; ditto, opaque to rays below G.

The author considers that these experiments show that the repulsion is not entirely due to the rays usually called heat, *i. e.* to the extreme and ultra red of the spectrum. Experiments have been tried with the electric and the solar spectrum formed with a quartz train, which prove the action to be also exerted by the luminous and ultra violet rays. Some numerical data have been obtained; but unfavourable weather has prevented many observations being made with the solar spectrum.

The barometric position of the neutral point dividing attraction from repulsion is next discussed. The position of this point varies with the density of the substance on which radiation falls, the ratio of its mass to its surface, its radiating and conducting-power for heat, the physical condition of its surface, the kind of gas filling the apparatus, the intensity of radiation, and the temperature of the surrounding atmosphere. The author is inclined to believe that the true action of radiation is repulsion at any pressure, and that the attraction observed when the rarefaction is below the neutral point is caused by some modifying circumstances connected with the surrounding gas, but not being of the nature of air-currents.

The neutral point for a thin surface of pith being low, and that for a moderately thick piece of platinum being high, it follows that at a rarefaction intermediate between these two points pith will be repelled, and that platinum will be attracted by the same beam of radiation. This is proved experimentally; and an apparatus showing simultaneous attraction and repulsion by the same ray of light is described and illustrated in the paper.

The paper concludes with a discussion of the various theories which have been adduced in explanation of these phenomena. The air-current and electrical theory are considered to have been abundantly disproved. The following experiment is given by the author to show that Prof. Osborne Reynolds's hypothesis of the movements due to evaporation and condensation at the surface will not account for all the facts of the case, and that therefore he has not hit upon the true explanation. A thick and strong bulb was blown at the end of a piece of very difficultly fusible green glass, specially made for steam-boiler gauges. In it was supported a thin bar of aluminium at the end of a long platinum wire. The upper end of the wire was passed through the top of the tube and well sealed in, for electrical purposes. The apparatus was sealed by fusion to the Sprengel pump, and exhaustion was kept going on for two days, until an induction-spark refused to pass across the vacuum. During this time the bulb and its contents were several times raised to a dull red heat. At the end of two days' exhaustion the tube was found to behave in the same manner as, but in a stronger degree than, it would in a less

perfectly exhausted apparatus, viz. it was repelled by heat of low intensity and attracted by cold. A similar experiment was next tried, only water was placed in the bulb before exhaustion. The water was then boiled away *in vacuo*, and the exhaustion continued, with frequent heating of the apparatus to dull redness, for about 48 hours. At the end of this time the bar of aluminium was found to behave exactly the same as the one in the former experiment, being repelled by radiation.

It is impossible to conceive that in these experiments sufficient condensable gas or vapour was present to produce the effects Prof. Osborne Reynolds ascribes to it. After the repeated heating to redness at the highest attainable exhaustion, it is impossible that sufficient vapour or gas should condense on the movable index to be instantly driven off by the warmth of the finger with recoil enough to drive backwards a heavy piece of metal.

While objecting to the theories already advanced as not accounting for all the facts of the case, the author confesses that he is not as yet prepared with one to put in their place. He wishes to avoid giving any theory on the subject until a sufficient number of facts have been accumulated. The facts will then tell their own tale. The conditions under which they invariably occur will give the laws, and the theory will follow without much difficulty.

Supplement. Received April 20, 1875.

Since the experiments mentioned in the foregoing Abstract were concluded, the author has examined more fully the action of radiation on black and white surfaces. At the highest exhaustion heat appears to act almost equally on white and on lampblacked pith, repelling them in about the same degree.

The action of the luminous rays, however, is different. These repel the black surface more energetically than they do the white surface. Taking advantage of this fact, the author has constructed an instrument which he calls a radiometer. This consists of four arms, suspended on a steel point resting on a cup, so that it is capable of revolving horizontally. To the extremity of each arm is fastened a thin disk of pith, lampblacked on one side, the black surfaces facing the same way. The whole is enclosed in a glass globe, which is then exhausted to the highest attainable point and hermetically sealed.

The author finds that this instrument revolves under the influence of radiation, the rapidity of revolution being in proportion to the intensity of the incident rays.

Several radiometers, of various constructions as regards details, but all depending on the above-named discovery, were exhibited by the author at the Soirée of the Royal Society on the 7th inst., and numerous experiments

were shown with them. The following Table, which gives the result of some experiments tried with one of the first-made radiometers (and therefore not so sensitive as more recent instruments), is copied from a card which was distributed during the evening:—

"Time required for One Revolution.

"Source of radiation.		Time in seconds.
"1 candle, 20 inches off	....	182
" 10 "	....	45
" 5 "	....	11
2 candles, 5 "	....	5
4 " 5 "	....	3
8 " 5 "	....	1·6
1 candle, 5 "	behind green glass ..	40
" 5 "	" blue " ..	38
" 5 "	" purple " ..	28
" 5 "	" orange " ..	26
" 5 "	" yellow " ..	21
" 5 "	" light red " ..	20
Diffused daylight, dull	....	2·3
" " bright	....	1·7
Full sunshine, 10 A.M.	....	0·3
" " 2 P.M.	....	0·25"

These experiments are not mentioned in the paper of which the above is an abstract, as it is intended to make the radiometer the subject of a future communication to the Society.

Mr. Lockyer communicated some particulars of the Eclipse of the Sun, April 6th, as observed at Bangkok, Siam.

April 29, 1875.

The DUKE OF DEVONSHIRE, K.G., Vice-President, in the Chair.

The Right Hon. W. E. Forster and the Right Hon. Russell Gurney were admitted into the Society.

The Right Hon. Sir James Colville, whose certificate had been suspended, as prescribed by the Statutes, was balloted for and elected a Fellow of the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Some Particulars of the Transit of Venus across the Sun, 1874, Dec. 9, observed on the Himalaya Mountains, Mussoorie, at Mary Villa."—Note II., with Appendix. By J. B. N. HENNESSEY, F.R.A.S. Communicated by Prof. STOKES, Sec. R.S. Received January 11, 1875.

1. The instruments used were the following:—

An altazimuth by Troughton and Simms, with an azimuth circle of 8 inches diameter read by three verniers, and a complete vertical circle, also of 8 inches, read by two verniers. The circles are divided to every 10′ of arc, and the verniers afford readings to 10″, or by estimation to at least 5″. The instrument is well provided with spirit-levels surmounted by scales. The altazimuth was used for fixing the station of observation and for determining time.

A mountain barometer.

A thermometer.

Four chronometers, viz.:—Dent 2047, used as a *journeyman* chronometer, and packed in wool in a wooden box by itself; Barraud 885, Dent 2775, and Arnold and Dent 758, were the three *fixed* chronometers, and were placed in the same case within a room where the temperature had only a moderate diurnal range (probably under 8°). The latter case was also well padded with wool and hemp, and these chronometers were never moved between 4th and 10th December.

The equatoreal has already been sufficiently described in Note No. I.

2. The point over which the equatoreal stood is called Venus Station. Its coordinates are as follows, and were determined by angles from two known points fixed by the Great Trigonometrical Survey of India, viz. Camel's Back and Vincent's Hill Stations:—

Venus Station.

Latitude N.	30° 27′ 36″·3
Longitude E. of Greenwich	78° 3′ 3″·2 $5^h\ 12^m\ 12^s{\cdot}2$
Height above sea	6765 feet.

3. Time was determined from the zenith-distances of α Tauri (east) and α Aquilæ (west), when these stars were (nearly) on the prime vertical. As a rule, four pairs of zenith-distances were taken to each star—a pair consisting of one observation-instrument face east and another face west, taken in rapid succession. The chronometer-time for each observation was obtained from transits over five horizontal wires; the resulting chronometer-error by *each* pair was computed. The journeyman chronometer was compared with the three fixed chronometers before and after observation, and the errors and rates of the latter were thus deter-

mined. Proceeding in this manner, and tabulating the *mean* of the results by the three fixed chronometers, it was determined :—

Mean Results.

Astronomical date.		Chronometer-reading.		Fast on local mean time.	Daily rate.	
		h m s		m s	s	
Dec. 4.	....	6 47 18·5	....	1 31·3		
„ 5.	....	6 48 40·6	....	1 37·7	+6·5	
„ 6.	....	6 37 19·1	....	1 45·1	+7·5	
„ 7.	....	Cloudy	....	—	+6·7	. . . (1)
„ 8.	....	6 42 12·1	....	1 58·6		
		18 56 17·4	....	2 2·2		
		23 17 19·4	....	2 3·5		
„ 9.	....	6 37 52·6	....	2 5·8	+7·3	

4. On the day of transit the journeyman was compared with the three fixed chronometers before and after ingress, and also before and after egress; and by employing the daily rate of $+7^{s}\cdot3$, and correcting the times of contact determined by the journeyman chronometer, we obtain :—

1874, Dec. 8.	Local mean time. h m s	
1st internal contact occurred	19 29 19·3	
2nd „ „ „	23 17 42·9	. . (2)
2nd external „ „	23 44 59·9	

where the tenths of seconds come into existence through the arithmetical processes involved. The original times were recorded merely to the nearest second, and I have no doubt that the latter degree of rigour was much in excess of what the phenomena were susceptible; so that the procedure adopted for determining time appears amply rigorous. Speaking roughly, $1''$ of Venus's diameter took 26^{s} to transit; and supposing that I could see $0''\cdot1$ of the former (which I doubt), each contact should have lasted, visibly, for say 3^{s}. But as regards the estimates of accuracy I actually made *immediately after* the events, I find it recorded that I thought my times may be true within the following limits :—

Time of 1st internal contact	$\pm 3^{s}$ to 4^{s}	
„ 2nd „ „	$\pm\ 2^{s}$	. . . . (3)
„ 2nd external „	$\pm\ 1^{s}$	

I am now, however, of opinion that these estimates (3) should be *increased* by one half.

5. From what has preceded, the Greenwich mean times of contact are :—

	h m s	
1st internal contact	14 17 7·1	
2nd „ „	18 5 30·7	. . . . (4)
2nd external „	18 32 47·7	

There is, however, an incongruity in (4); for in this reduction the longitude of Venus Station is taken east of Greenwich by $5^h\ 12^m\ 12^s{\cdot}2 = H$, the *origin* for Indian longitude being adopted at $5^h\ 20^m\ 57^s{\cdot}3$ E. for Madras Observatory. In reality, however, H refers not to the *local meridian* necessarily adopted in (2), but to a concluded meridian of origin adopted for the principal triangulation of the Great Trigonometrical Survey at Kalianpur, whereas the times (2) refer to the *local* meridian, as already stated. Denoting the value of H corresponding to the local meridian by H_1, we may find (nearly) $H_1 - H = h^s$ thus:—Let A_0 denote the azimuth of a terrestrial point P as determined by observations to a circumpolar star about its elongation, and A_c the corresponding value as brought up by the triangulation from the concluded meridian of origin; also let $A_0 - A_c = a''$, and let λ stand for the latitude of P; then it can be seen that, as a correction to (4),

$$h^s = \frac{-a''}{15} \operatorname{cosec} \lambda.$$

Now a'' is not known at Venus Station; but at Banog Station, distant 2·9 miles W.N.W., $a'' = -14''{\cdot}54$. Adopting this value, we find $h^s = +1^s{\cdot}9$, and the *true* Greenwich mean times of contact become

	h	m	s	
1st internal contact	14	17	9·0	} .... (5)
2nd " "	18	5	32·6	
2nd external "	18	32	49·6	

The results (5) are given in supercession of (4).

Dehra Doon, 17th Dec., 1874.

[The "better sketch" referred to in the author's footnote to Note No. I. (p. 256) arrived in time to appear in that Note, and is, in fact, the sketch from which the woodcut on p. 257 was copied.—G. G. S.]

"Appendix to Notes on Transit of Venus across the Sun." Received January 11, 1875.

After posting my Note No. I., describing the phenomena I had observed at Mussoorie, I received, on 12th December, 1874, a communication from my friend the Rev. H. D. James, M.A., describing briefly what he had seen at his station of observation. In reply I made inquiry on some additional points, to which he replied on 14th inst., so that his letter has just reached me. Mr. James was located at Chakrata, on the Himalaya Mountains, at a height of 7300 feet above the sea, in lat. N. 30° 43′, long. E. 77° 54′. His station is distinctly visible from Mussoorie on a clear day. The following facts are taken from his letters above mentioned, and appear to deserve being recorded, more particularly from

he circumstance that but few observers of the transit are likely to have been placed at considerable heights above the sea.

Mr. James was attended by his son Henry, a young gentleman with plenty of intelligence and a commendable spirit of inquiry. The instrument used was a telescope by Smith and Beck, the property of Mr. James; object-glass $3\frac{1}{2}$ inches and its focal length 4 feet; at ingress, eyepiece 60-power, and field a neutral tint; at egress, eyepiece 100-power, and field red. For timepiece he used his pocket-watch, which has a seconds-hand. The watch "gained considerably, perhaps a minute in 12 hours."

Under date 9th December, 1874, Mr. James states:—

"When she (*i. e.* Venus) was about halfway on (at ingress) the sun we both noticed a fringe of white light illuminating that rim of the planet which was yet on the dark sky. When she went off we noticed the same fringe of light, but for a much shorter time, and when only about one eighth of her had passed the sun's disk."

His watch times of contact were as follows:—

	h	m	s
1st internal contact	7	41	20
2nd ,, ,, ...	11	30	15
2nd external ,, ...	11	57	25

After receipt of the preceding, I wrote to Mr. James, as already mentioned. I stated briefly that my view resembled his; that I had seen a ring of light, but no "pear-drop" or other ligament, at internal contacts. I gave him rough sketches of some of the ligaments described in 1769, and inquired if he had seen any thing like them. I also asked him to describe the fringe of light he had observed more particularly.

In reply, Mr. James states, under date 14th December:—

"When about half her orb had entered (alluding to ingress) my attention was attracted to the other half yet on the dark sky: to me it was dark; hence I infer that my field was not so light as it ought to have been. Its outline, up to this time quite invisible to me, became now illumined with a fringe of white light. I then also noticed a much fainter, thinner, edging of light on the outline of the limb on the sun's disk, which soon ceased to be visible. The fringe external was rather less in width than $\frac{1}{64}$ of the planet's diameter. * * * * The light somewhat resembled that which we see so plainly in India lighting up the dark side of the moon three or four days old; but it was brighter, not diffusive as that is, its inner edge being clearly marked. It remained visible as long as there was any appreciable portion of the planet beyond the sun's circumference.

"As the time for the internal contact approached, that half of the planet which was still entering appeared to lose its semicircular shape and to become oval. I compared it to the thinner half of an egg; but, since, I have examined several eggs, and find that my comparison would represent a distortion greater than I had intended. Just before the con-

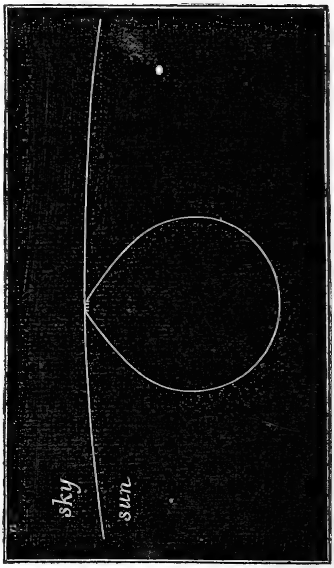
tact ceased, the end of the oval seemed as it were adhering to the sun's edge, and could not get free, rendering it difficult to decide when the contact ceased. Another impediment in the way of accurate timing was, that the outline of Venus looked woolly and wave-like, from a very annoying tremor in the air. Hence the notes we entered were, 'Internal contact ceased $7^h\ 41^m\ 20^s$, quite clear $7^h\ 42^m$.' As to the ligament which seemed to knit the two edges together, I am disposed to attribute it solely to the billowy motion of the planet's outline; for it had a hairy appearance, and sunlight could be seen through it.

"In timing the remaining contacts there was no difficulty, for as the sun arose Venus appeared to diminish in size, her outline becoming sharply defined.

"At egress the oval shape did not reappear; but just at the moment of internal contact there was a sort of flickering movement, as if the planet's edge had touched, withdrawn, and touched again. This was at $11^h\ 30^m\ 15^s$. At $11^h\ 33^m\ 27^s$, when nearly one eighth of her orb had crossed the border, its outline was for a brief while fringed with an edging of light.

"The flickering movement just mentioned, evidently an ocular illusion, induced by the eye's weariness from intent gazing, was again noticed at $11^h\ 57^m\ 25^s$, when the external contact ceased."

Writing again on 15th December, 1874, Mr. James enclosed the sketch given in margin, remarking in his letter, "What I meant to express by 'ligament,' was the point of connexion formed by the boundary line itself, which appeared adhesive and at the same time hairy. The dots I have placed at the point are *within* the line. I saw no 'black drop,' if by that is intended any thing beyond and attached to the boundary line of Venus. I should therefore have expressed myself more accurately if, instead of 'ligament,' I had written 'the point of apparent adhesion looked hairy.'"



As Venus appeared at $7^h\ 41^m\ 20^s$, as the internal contact was in the act of ceasing.

It will be seen that both Mr. James and I observed an edging of light around the dark limb of Venus, and that we agree that it was quite distinct at ingress and less plain at egress. *I* saw this edging decidedly as an *annulus*, and, as stated in Note No. I., it was continued round the bright limb. The complete ring thus presented to view was plainly a visible

atmosphere or envelope. Notwithstanding the somewhat conflicting statements which have appeared since writing my Note No. I., as to the phenomena seen at certain stations, and pending the authoritative decisions which may hereafter be pronounced, it appears to me probable that height of station, inferiority of instruments, insensibility of eye to faint lights and shadows, and other causes, are likely to conspire towards producing a ligament (or *pear*-drop) at the internal contacts, provided there is an atmosphere or envelope around the planet to afford a *first* cause.

Dehra Doon,
17th December, 1874.

II. "On a Continuous Self-registering Thermometer." By W. Harrison Cripps. Communicated by Prof. Stokes, Sec.R.S. Received March 17, 1875.

The object of this paper is to explain the working of a continuous self-registering thermometer, the detail of which I have been for some years endeavouring to perfect, though it is only recently that I have brought my instrument to the perfection necessary for practical demonstration.

I am aware of many imperfections in the instrument, but trust that these will be remedied after some further experiments. The object of my invention is to produce a thermometer that shall not only register the highest and lowest temperature occurring in a given time, but shall also, by a pencil indicator, mark automatically every minute variation occurring during the time the instrument is at work.

This thermometer will, I hope, be of value for meteorological observations; for it will show the exact variations taking place from minute to minute during day and night, and by this means determine the exact amount of alterations in temperature occurring simultaneously at various places during different periods of the month or year, without the trouble of photographic registration.

By a simple modification of my instrument, I believe that I shall be able to produce a thermometer of such form and size as shall enable the physician or physiologist to learn with accuracy the exact hour and minute when any alteration of temperature in the human body takes place, and automatically to register these variations.

As regards the barometer, many beautiful self-registering instruments are in use; for the lower surface of the mercurial column being exposed, offers an easy method of producing a motive force. In the thermometer the case is different, for no means of communication exist between the registering machinery and the mercury in its hermetically sealed tube. So far as I know, the only instrument of this kind ever suggested is the one by Mérey; but from the fact that air is the motive force used by

him, acting upon mercury in an open tube, his instrument must be regarded more as a *barometer* than as a means of measuring the temperature of the atmosphere.

My instrument is divided into two portions :—1st, the thermometer, which marks the degrees; 2ndly, the clockwork, which indicates the hours and minutes.

The thermometer shall be first described. The form in which it was originally made, and which perhaps serves best for illustrating the principle, was the following :—

A glass bulb, rather more than an inch in diameter, ends in a glass tube 12 inches long, having a bore of $\frac{1}{8}$ inch. This tube is coiled round the bulb in such a manner as to form a complete circle 4 inches in diameter, the bulb being in the centre of this circle.

Fixed to opposite poles of the bulb, exactly at right angles to the encircling tube, are two needle-pointed pivots. These pivots work in minute metal depressions fixed to the sides of two parallel uprights.

It will be seen from this arrangement that the bulb with its glass tube will rotate freely between the uprights, and the pivots will be the centre of a circle, the circumference of which is formed by the glass tube.

The bulb is filled with spirit in such quantity that at 60° Fahrenheit the spirit will fill not only the bulb, but about 4 inches of the tube. Mercury is then passed into the tube till it comes into contact with the spirit, and in such quantity as to fill up about 3 inches of the remaining portion of the tube.

The spirit is now heated to 120°, and as it expands forces the column of mercury in front of it till the mercury comes within $\frac{1}{4}$ inch of the end of the tube. The tube is then hermetically sealed, enclosing a small quantity of air.

If the thermometer be now arranged with its needle-points between the uprights, it will be observed that, as the spirit contracts on cooling, it draws the column of mercury with it. This immediately alters the centre of gravity, and the bulb and tube begin to revolve in a direction opposite to that of the receding mercury.

On again applying heat, and the mercury passing forwards, the bulb regains its original position.

By this simple arrangement, the two forces, heat and gravity, acting in contrary directions, generate a beautifully steady rotatory movement.

The method by which this movement is made serviceable for moving the register will now be described.

A grooved wheel, 2 inches in diameter, is fixed to one of the central pivots, therefore revolving with the bulb. Directly above and at a distance of 7 inches from this wheel is fixed between needle-points another wheel of exactly similar size. Around and between these two wheels passes a minute endless chain.

To the chain is fixed a tiny pencil, which will be carried backwards and forwards between the wheels in a perpendicular line.

This constitutes the register worked by the thermometer.

The clockwork portion of the machine is so arranged that it causes a vertical cylinder, 4 inches diameter and 5 inches in length, to revolve once in 24 hours. Round this cylinder is fixed a piece of paper 12 inches long, 5 inches wide. On the paper in the direction of its greatest length are ruled 100 lines, $\frac{1}{20}$ inch apart, each indicating 1° Fahrenheit. Across the paper, at right angles to these lines, are ruled twenty-four lines in dark ink, indicating the hours; between these three others, more lightly marked, for the quarters. The cylinder is so placed that, as it revolves, the surface of the paper is $\frac{1}{10}$ of an inch away from the point of the pencil register moving at right angles to its surface.

A small striker is connected with the clockwork in such a manner that every 5 minutes (or oftener if required) it gives the pencil a gentle tap, thus striking its point against the paper.

By this means all friction of the moving pencil against the paper is avoided, and the index is marked by a series of dots.

In the model which I bring before the Society, the arrangement of the bulb is somewhat modified, for a spiral coil of glass tube is made to take its place. By this means not only is a larger surface of the spirit exposed to the air, making the thermometer more delicate, but also all fear of the mercury being drawn into the bulb is obviated.

The last turn of the spiral coil is made as a perfectly true circle with a diameter of 4 inches.

The spiral within this circle is made of such a size that each degree of heat applied to the contained spirit shall force the mercury $\frac{1}{10}$ of an inch along the circle.

Since the wheel moving the register equals 2 inches diameter, or only half the diameter of the mercurial circle, the pencil will move $\frac{1}{20}$ of an inch for each degree of heat.

In some of the earlier experiments performed, I found that in straight tubes the spirit had a tendency to pass the mercurial column without moving it. This, however, is not the case when the tube forms a portion of a circle, for the pressure of the mercury at the lowest portion of the segment prevents the spirit passing the mercury.

I trust that I have made this description of the thermometer intelligible; but a glance at the working model which I have the honour of bringing before the Society will at once explain the details of the machine.

The Society then adjourned over Ascension Day, to Thursday, May 13th.

Presents received, April 22, 1875.

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"The Theory of the Division of the Octave, and the Practical Treatment of the Musical Systems thus obtained. Revised Version of a Paper entitled 'On Just Intonation in Music; with a description of a new Instrument for the easy control of Systems of Tuning other than the equal Temperament of 12 Divisions in the Octave. By R. H. M. Bosanquet, Fellow of St. John's College, Oxford. Communicated by H. J. Stephen Smith, F.R.S., Savilian Professor of Geometry in the University of Oxford. Received Dec. 4, 1872. Read Jan. 30, 1873*.'" Received November 24, 1874.

Contents.

The mode of expressing Intervals.

In the original paper presented by the writer to the Royal Society, logarithms were employed as the measure of intervals, as they have been

* See 'Proceedings,' vol. xxi. p. 131.

commonly employed by others. Great advantages have been found, however, to result from the adoption of the equal temperament (E. T.) semitone, which is $\frac{1}{12}$ of an octave, as the unit of interval. It is the unit most familiar to musicians, and has been found to admit of the expression of the theory of cyclical systems by means of formulæ of the simplest character. The writer therefore devised the following rules for the transformation of ratios into E. T. semitones and *vice versâ*, and subsequently found that De Morgan had given rules for the same purpose which are substantially the same (Camb. Phil. Trans. vol. x. p. 129). The rules obviously depend on the form of log 2. The form of the first rule affords a little more accuracy than De Morgan's.

Rule I. To find the equivalent of a given vibrations-ratio in E. T. semitones.

Take log (ratio), subtract $\frac{1}{300}$, and call this the first improved value. From log (ratio) subtract $\frac{1}{300}$ of the first improved value and $\frac{1}{10{,}000}$ of the first improved value. Multiply the remainder by 40. We can rely on five places in the result.

The following data are introduced here; they can be verified by numbers given in Woolhouse's tract:—

$$\text{Fifth} = 7{\cdot}019{,}550{,}008{,}654.$$
$$\text{Third} = 4 - {\cdot}136{,}862{,}861{,}351.$$

Five places are ordinarily sufficient.

Rule II. To find the vibrations-ratio of an interval given in E. T. semitones.

To the given number add $\frac{1}{300}$ and $\frac{1}{10{,}000}$ of itself. Divide by 40. The result is the logarithm of the ratio required. We can rely on five places in the result, or on six, if six are taken.

Ex. The E. T. third is 4 semitones. The vibrations-ratio found as above is 1·259921.

Hence the vibrations-ratio of the E. T. third to the perfect third is very nearly 126 : 125.

Definitions.

Regular systems are such that all their notes can be arranged in a continuous series of equal fifths.

Regular cyclical systems are not only regular, but return into the same pitch after a certain number of fifths. Every such system divides the octave into a certain number of equal intervals.

Error is deviation from a perfect interval.

Departure is deviation from an E. T. interval.

Intervals taken upwards are called positive, taken downwards, negative.

Systems are said to be *of the* r*th order*, positive or negative, when the departure of 12 fifths is $\pm r$ units of the system.

Intervals formed by Fifths.

When successions of fifths are spoken of, it is intended that octaves be disregarded. If the result of a number of fifths is expressed in E. T. semitones, any multiples of 12 (octaves) are cast out. Representing the fifth of any system by $7+\delta$, where δ is the departure of one fifth expressed in E. T. semitones, we form the following intervals amongst others :—

Departure of 12 fifths $=12\delta$

$$(12\times(7+\delta)=84+12\delta, \text{ and } 84 \text{ is cast out}).$$

Two-fifths tone $=2+2\delta$

$$(2\times(7+\delta)=14+2\delta, \text{ and } 12 \text{ is cast out}).$$

Seven-fifths semitone, formed by seven fifths up, $=1+7\delta$

$$(7\times(7+\delta)=49+7\delta, \text{ and } 48 \text{ is cast out}).$$

Five-fifths semitone, formed by five fifths down, $=1-5\delta$

$$(5\times-(7+\delta)=-(35+5\delta), \text{ and } 36 \text{ is added}).$$

The seven-fifths semitone will be denoted by s $(=1+7\delta)$; the five-fifths semitone by f $(=1-5\delta)$.

Regular Systems.

The importance of regular systems arises from the symmetry of the scales which they form.

Theorem α. In any regular system five seven-fifths semitones + seven five-fifths semitones make an exact octave, or $5s+7f=12$.

For the departures (from E. T.) of the 5 seven-fifths semitones are due to 35 fifths up, and those of the 7 five-fifths semitones to 35 fifths down, leaving 12 E. T. semitones, which form an exact octave; or,

$$5(1+7\delta)+7(1-5\delta)=12.$$

Theorem β. In any regular system the difference between the seven-fifths semitone and the five-fifths semitone is the departure of 12 fifths, having regard to sign; or,

$$s-f=\text{departure of 12 fifths}.$$

Let δ be the departure of each fifth of the system, then $s=1+7\delta$, $f=1-5\delta$; whence $s-f=12\delta$.

Regular Cyclical Systems.

The importance of regular cyclical systems arises from the infinite freedom of modulation in every direction which is possible in such systems when properly arranged; whereas in non-cyclical systems required modulations are liable to be impossible, owing to the demand for notes lying outside the material provided.

Theorem i. In a regular cyclical system of the $\pm r$th order the difference between the seven-fifths semitone and five-fifths semitone is $\pm r$ units of the system, or $s-f=\pm r$ units.

Recalling the definition of rth order ($12\delta=\pm r$ units), the proposition follows from Th. β.

Cor. This proposition, taken with Th. α, enables us to ascertain the number of divisions in the octave in systems of any order, by introducing the consideration that each semitone must consist of an integral number of units. The principal known systems are here enumerated:—

7-fifths semitone $=x$ units.	5-fifths semitone $=y$ units.	Number of units in octave (Th. α) $5x+7y=n$.
Primary (1st order) Positive.		
2	1	17
3	2	29
4	3	41
5	4	53
6	5	65
Secondary (2nd order) Positive.		
11	9	118
Primary Negative.		
1	2	19
2	3	31
Secondary Negative.		
3	5	50

Theorem ii. In any regular cyclical system, if the octave be divided into n equal intervals, and r be the order of the system, the departure of each fifth of the system is $\frac{r}{n}$ E. T. semitones.

For departure of 12 fifths $=12\delta=r$ units by definition and the unit$=\frac{12}{n}$ E. T. semitones;

$$\therefore \delta=\frac{r}{n}.$$

Theorem iii. If, in a system of the rth order, the octave be divided into

n equal intervals, $r+7n$ is a multiple of 12, and $\frac{r+7n}{12}$ is the number of units in the fifth of the system.

Let ϕ be the number of units in the fifth.

Then $\phi\,\frac{12}{n}=7+\delta=7+\frac{r}{n}$;

$$\therefore\ \phi=\frac{7n+r}{12},$$

and ϕ is an integer by hypothesis; whence the proposition.

Cor. From this proposition we can deduce corresponding values of n and r. It is useful in the investigation of systems of the higher orders. Casting out multiples of 12, where necessary, from n and r, we have the following relations between the remainders:—

Remainder of											
n	1	2	3	4	5	6	7	8	9	10	11
r	5	10	3	8	1	6	11	4	9	2	7

Theorem iv. If a system divide the octave into n equal intervals, the total departure of all the n fifths of the system $=r$ E. T. semitones, where r is the order of the system.

For by Th. ii. $\delta=\frac{r}{n}$; whence

$$n\delta=r,$$

or the departure of n fifths $=r$ semitones.

This gives rise to a curious mode of deriving the different systems.

Suppose the notes of an E. T. series arranged in order of fifths, and proceeding onwards indefinitely, thus:—

$$c\ g\ d\ a\ e\ b\ f\sharp\ c\sharp\ g\sharp\ d\sharp\ a\sharp\ f\ c\ g\ \ldots,$$

and so on. Let a regular system of fifths start from c. If they are positive, then at each step the pitch rises further from E. T. It can only return to c by sharpening an E. T. note.

Suppose that b is sharpened one E. T. semitone, so as to become c; then the return may be effected

at the first b in 5 fifths,
at the second b in 17 fifths,
at the third b in 29 fifths; and so on.

Thus we obtain the primary positive systems. Secondary positive systems may be got by sharpening $b\flat$ 2 semitones; and so on.

If the fifths are negative, the return may be effected by depressing $c\sharp$ a semitone in 7, 19, 31 . . . fifths; we thus obtain the primary negative systems; or by depressing d two semitones, by which we get the secondary negative systems; and so on.

An instructive illustration may be made as follows; it requires too large dimensions for convenient reproduction here :—

Set off on the axis of abscissæ the equal temperament series in order of fifths, as above, taking about 10 complete periods. If the distances of the single terms are made 1 centimetre, this will take $1^{\text{m}}\cdot20$ in length, starting from the origin on the left.

Select a unit for the E. T. semitone of departure, say 1 decimetre.

Rule a series of lines parallel to the axis of abscissæ, at distances representing integral numbers of E. T. semitones, both above and below.

Rule, parallel to the axis of ordinates, straight lines through the points representing the E. T. notes.

Enter on the intersections the names of the E. T. notes they represent. Thus the notes on the positive ordinate of *c* are *c*–*c*♯–*d* . . ., and so on, each pair separated by 1 decimetre, and the notes on the negative ordinate of *c* are *c*–*b*–*b*♭ . . . .

If we then join the *c* on the left hand of the axis of abscissæ to all the other *c*'s on the figure, except, of course, those on the axis, we obtain a complete graphic representation of all the systems whose orders are included. The rth order is represented by lines drawn to the *c*'s in the rth line above, the $-r$th by the lines drawn to the *c*'s in the rth line below.

This illustration brings specially into prominence the singularity of multiple systems, as all the multiples of any system lie on the same straight line with it, and the representation fails to give all the notes of such systems.

Multiple Systems.

Multiple systems are such that the number of divisions in the octave (kn) in any such system is a multiple (k) of the number of divisions (n) of some other system.

Multiple systems have not been as yet practically applied.

These systems are not strictly regular; for though their fifths are all equal, yet they do not form one continuous series, but several. They are strictly cyclical, *i. e.* they divide the octave into n equal intervals.

Theorem v. A multiple system, kn, may be regarded as being of order kr, where n is a system of order r.

For, n being a system of order r, $r+7n$ is a multiple of 12; $\therefore$ also $k(r+7n)$ is a multiple of 12, which is the condition that the system kn be of order kr.

This is useful in the investigation of systems of the higher orders.

If n is a multiple of 12, the system is a multiple of the E. T., and of order zero.

In the illustration described under Th. iv. the notes of a multiple system (kn) are the same as those of system n, until the latter is complete. The rest of the representation consists simply of the same notes

repeated over and over again. To obtain the rest of the notes we should have to change the starting-point.

On the whole, we may regard the system kn as consisting of k different systems n, having starting-points distant from each other by $\frac{1}{k}$ of the unit of the system n.

It follows immediately that the system kn is of the krth order; for in every unit of the system n there are k units of system kn; and so in r units of system n there are kr units of system kn.

Any system, when n is not a prime, can be regarded as a multiple system.

Thus the system of 59 is of the 7th order; 118 consequently a multiple system of the 14th order, in which point of view it is of no interest; but, casting out the 12 from the order, it may be also regarded as an independent system of the 2nd order, in which point of view it is of considerable interest.

Formation of Major Thirds in Positive and Negative Systems.

The departure of the perfect third is $-{\cdot}13686$. Hence negative systems (where the fifth is $7-\delta$) form their thirds in accordance with the ordinary notation of music. For if we take 4 negative fifths up, we have a third with negative departure (-4δ) which can approximately represent the departure of the perfect third. Thus $c\sharp$ is either the third to a, or four fifths up from a, in accordance with the usage of musicians.

Positive systems form their thirds by 8 fifths down; for their fifths are of the form $(7+\delta)$, and 8 fifths down give the negative departure (-8δ). Thus the third of a should be $d\flat$, which is inconsistent with musical usage. Hence positive systems require a separate notation. Helmholtz proposed a notation for this purpose, which, however, is unsuitable for use with written music. The following notation is here adopted for positive systems in general; it is not intended to be limited to any one system, like Helmholtz's. In fact it may, on occasions, be used even for negative systems.

Notation for Positive Regular Systems.

The notes are arranged in series, each containing 12 fifths, from $f\sharp$ up to b. These may be called duodenes, adopting a term introduced by Mr. Ellis. The duodene

$$f\sharp\text{–}c\sharp\text{–}g\sharp\text{–}d\sharp\text{–}a\sharp\text{–}f\text{–}c\text{–}g\text{–}d\text{–}a\text{–}e\text{–}b,$$

which contains the standard c, is called the unmarked duodene. No distinction is made in these series between such notes as $c\sharp$ and $d\flat$. These signs refer only to the E. T. note from which the note in question is derived; the place in the series of fifths is determined by the notation.

Continuing the series to the right, each note of the next 12 fifths is affected with the mark / (mark of elevation), drawn upwards in the direction of writing. These notes join on to the unmarked duodene as follows :—

e–b–/f♯–/c♯–/g♯ . . . .,

and so on.

Thus /*c* is 12 fifths to the right of *c*, and the interval /*c–c* is the departure of 12 fifths.

The next duodene to the right is affected with the mark //, which joins on to the last as before :—

/e–/b–//f♯ . . . .,

and so on.

Proceeding in the same way, we have notes affected with such marks as ///, ////.

Return to the unmarked duodene, and let it be continued to the left; the notes in the next duodene on the left are affected with the mark \, (mark of depression), drawn downwards in the direction of writing. The junction with the unmarked duodene will be

\c–\g–\d–\a–\e–\b–f♯–c♯ . . . .

The next junction on the left will be

\\e–\\b–\f♯ . . . .;

and, proceeding in the same way, we have such marks as \\\, \\\\.

Thus *c*–*e* is a major third determined by eight fifths down in the whole series; and *e* will have the departure (-8δ) from the E. T. note *e* derived from *c*.

Notation applicable to all Regular Systems, Negative as well as Positive.

As this notation simply consists of a determination of position in a continuous series of fifths, it may be applied to all regular systems, positive or negative; but, as it is not commonly needed for negative systems, it is not generally applied to them.

Formation of Harmonic Sevenths in Positive and Negative Systems.

The harmonic seventh is the interval whose ratio is 7 : 4. It affords a smooth combination, free from beats.

The departure of the harmonic seventh from the note which gives the E. T. minor seventh is —·31174 (Rule I.).

Helmholtz observes that his system of just intonation affords an approxi-

mation to the harmonic seventh. In fact, if we form a seventh by 14 fifths down in positive systems (fifth$=7+\delta$), we obtain a note with negative departure (-14δ), which can approximately represent the harmonic seventh: c–$\diagdown b\flat$ represents such an interval.

Mr. Ellis has observed (Roy. Soc. Proc. 1864) that the mean-tone system, which is negative, affords a good approximation to the harmonic seventh. In fact, if we form a seventh by 10 fifths up in negative systems (fifth$=7-\delta$), we obtain a note with negative departure (-10δ), which can approximately represent the harmonic seventh.

Concords of Regular and Regular Cyclical Systems.

These considerations permit us to calculate the departures and errors of concords in the various regular and regular cyclical systems. There is, however, one quantity which may be also conveniently taken into consideration in all cases, viz. the departure of 12 fifths of the system. We will call this Δ, putting $\Delta=12\delta$.

We have then the following Table of the characteristic quantities for the more important systems hitherto known.

The value of the ordinary comma $\left(\frac{81}{80}\right)$ is ·21560. It is comparable with the values of Δ, and if introduced in its place in the Table would give rise to a regular non-cyclical system, lying between the system of 53 and the positive system of perfect thirds, the condition of which would be that the departure of 12 fifths$=$a comma.

Name, or n.	Order, r.	$\Delta=12\delta$, or $12\frac{r}{n}$.	Error of fifth, $\delta-\cdot01955$.	Error of third, $\cdot13686-8\delta$.	Error of harmonic seventh, $\cdot31174-14\delta$.
17	1	·70588	·03927	−·33373	−·51178
29	1	·41379	·01493	−·27586	−·17101
41	1	·29268	·00484	−·19512	−·02970
Perfect fifths.	...	·23460	...	−·01954	·03804
53	1	·22642	−·00068	−·01409	·04758
Positive perfect thirds.	...	·20529	−·00244	...	·07223
118	2	·20339	−·00260	·00127	·07445
65	1	·18462	−·00417	·01378	·09635
($\delta=\frac{r}{n}$ is here negative.)				$\cdot13686+4\delta$.	$\cdot31174+10\delta$.
43	−1	−·29707	−·04431	·03784	·06418
31	−1	−·38710	−·05181	·00783	−·01084
Mesotonic. Negative perfect thirds.	...	−·41058	−·05376	...	−·03041
50	−2	−·48000	−·05955	−·02314	−·08826
19	−1	−·63158	−·07218	−·05367	−·21458

A few systems of the higher orders, which possess some interest, will be given separately.

An illustration may be made as follows, which shows on inspection all the data involved in the above Table, and the properties of any other system introduced into it.

Take axes of abscissæ and ordinates, and set off on both distances representing tenths of E. T. semitones—for ordinary purposes 10 inches to the E. T. semitone answers best; for Lecture scale, 1 metre to the E. T. semitone.

On the axis of ordinates set off points representing the values in column Δ of the Table, and corresponding values for any other system required. Through each of these points rule a straight line parallel to the axis of abscissæ.

On the axis of abscissæ set off points representing the values $-{\cdot}13686$ and $-{\cdot}31174$. Rule lines through these parallel to the axis of ordinates. These abscissæ represent respectively perfect thirds and perfect sevenths.

Draw lines inclined to the axis of abscissæ at angles $\tan^{-1}\frac{3}{2}$ and $\tan^{-1}\frac{6}{7}$. These give, by their intersections with the lines of the different positive systems, the thirds and sevenths respectively.

Draw lines inclined to the axis of abscissæ at angles $\tan^{-1}-3$ and $\tan^{-1}-\frac{6}{5}$. These give, by their intersections with the lines of the different negative systems, the thirds and sevenths respectively.

The errors of the thirds and sevenths are the perpendicular distances of the intersections which determine them from the ordinates of perfect thirds and sevenths already constructed.

In Regular Cyclical Systems, to find the number of Units in any Interval in the Scale.

Let x be the number of units in the seven-fifths semitone, then

$$x \cdot \frac{12}{n} = 1 + 7\delta = 1 + 7\frac{r}{n},$$

or

$$x = \frac{n+7r}{12}.$$

It is easy to see that x will always be integral if the order condition is satisfied (Th. iii.), viz. if $7n+r$ is a multiple of 12.

For then $7(7n+r)=49n+7r$; whence, casting out $48n$, $n+7r$ is a multiple of 12.

We can now determine the remaining intervals in terms of x and r:—

Interval.	No. of units. Positive systems.	Negative systems.
5-fifths semitone	$x - r$	$x - r$
Minor tone / 10-fifths tone	$2x - 2r$	..
Major tone / 2-fifths tone	$2x - r$	$2x - r$
Minor third	$3x - r$	$3x - 2r$
Major third	$4x - 3r$	$4x - 2r$
Fourth	$5x - 3r$	$5x - 3r$
Fifth	$7x - 4r$	$7x - 4r$
Sixth	$9x - 6r$	$9x - 5r$
Harmonic seventh	$10x - 7r$	$10x - 5r$
Major seventh	$11x - 7r$	$11x - 6r$
Octave	$12x - 7r$	$12x - 7r = n$.

The $-r$'s in negative systems are, of course, positive quantities.

Employment of Positive Systems in Music.

Rule for thirds.—If we write down one of the duodenes of the notation,

$$f\sharp - c\sharp - g\sharp - d\sharp - a\sharp - f - c - g - d - a - e - b,$$

and remember that positive systems form their thirds by 8 fifths down, we have the rule :—

The four accidentals on the left in any duodene of the notation form major thirds to the four notes on the extreme right in the same duodene. All other notes have their major thirds in the next duodene below. Thus d–$f\sharp$, c–$\diagdown e$ are major thirds.]

Use of the Notation with Musical Symbols.

It is an essential point in this notation that it can be used with musical symbols. The following example shows the major and minor chords and the interval used for the harmonic seventh :—

The first chord is the major triad ; the second involves g–$\diagdown f$, the harmonic seventh ; the fourth crotchet gives the minor common chord ; and the first chord of the second bar is the sharp sixth, rendered peculiarly smooth by employment of the approximate harmonic seventh for the interval $\diagup a\flat$–$f\sharp$.

The employment of positive systems is presupposed with this notation, unless the contrary is expressly stated.

Such passages as this can be played on the harmonium hereafter described.

Principle of Symmetrical Arrangement in Regular Systems.

If we place the E. T. notes in the order of the scale, and set off the departures of the notes of any regular system at right angles to the E. T. line, sharp departures up and flat departures down, we obtain the positions of what may be called a symmetrical arrangement.

The distances of the E. T. notes from the starting-point are abscissæ, and the departures ordinates.

Positive Systems.

The subjoined is a symmetrical arrangement of the notes of General Thompson's enharmonic organ (p. 402). It is selected as not being too extensive for reproduction, as being of historical interest, and as illustrating the nature of the difficulty caused by the distribution of such systems into separate key-boards. Each of the single vertical steps represents the departure of one fifth.

The property of symmetrical arrangements, from which they derive their principal importance, is that, position being determined only by relations of interval, the notes of a combination forming given intervals present always the same form, whatever be the key or the actual notes employed.

Let us express, as before, the number of E. T. semitones, which is now our abscissa, by simple integers, and the number of departures of fifths, which is our ordinate, by a coefficient attached to δ. Then we have only to note the values of the different intervals to obtain their coordinates with respect to any note taken as origin.

Thus the third is $4-8\delta$, or four steps to the right and eight down (c–$\diagdown e$); the fifth is $7+\delta$, seven steps to the right and one up (c–g); the minor third is three to the right and nine up ($\diagdown e$–g); and so on.

Two notes are omitted from the otherwise complete series, b and $\diagdown\diagdown d$; and we notice the number of otherwise complete chords which their absence destroys.

Distribution over three Key-boards.—As an example of the effect of this, we note that the notes of the chord of *a minor* are all present; but they are $a_{1.2}$–$/c_1$–e_2, so that the third and fifth are on different key-boards.

Negative Systems.

According to the enunciation of the principle of symmetrical arrangement, the positions should be taken lower for negative systems as we ascend in the series of fifths; but it is practically more convenient to use the positive form in negative systems as well. The coordinates of some intervals become different—the third is $4+4\delta$, the minor third $3-3\delta$, &c.

Symmetrical Arrangement of the Notes of Thompson's Enharmonic Organ.

The subscripts $_{1,2,3}$ refer to its three key-boards.

12.	/c_1	.	.	.	.	.	.	.	.	.	.	.	/c_1
11.	.	.	.	.	.	/f_1	.	.	.	.	.	.	.
10.	.	.	.	.	.	.	.	.	.	.	/$b\flat_1$	.	.
9.	.	.	.	/$e\flat_1$	.	.	.	.	.	.	.	.	.
8.	.	.	.	.	.	.	.	.	/$a\flat_1$	.	.	.	.
7.	.	/$c\sharp_1$	.	.	.	.	.	.	.	.	.	.	.
6.	.	.	.	.	.	.	/$f\sharp_1$	.	.	.	.	.	.
5.													
4.	.	.	.	.	e_2	.	.	.	.	.	.	.	.
3.	.	.	.	.	.	.	.	.	.	$a_{1,2}$	.	.	.
2.	.	.	$d_{1,2}$	.	.	.	.	.	.	.	.	.	.
1.	.	.	.	.	.	.	.	$g_{1,2,3}$	.	.	.	.	.
12.	$c_{1,2,3}$	.	.	.	.	.	.	.	.	.	.	.	$c_{1,2,3}$
11.	.	.	.	.	.	$f_{1,2,3}$	.	.	.	.	.	.	.
10.	.	.	.	.	.	.	.	.	.	.	$b\flat_{1,3}$	.	.
9.	.	.	.	$e\flat_{1,3}$	.	.	.	.	.	.	.	.	.
8.	.	.	.	.	.	.	.	.	$a\flat_3$	.	.	.	.
7.	.	$c\sharp_2$	.	.	.	.	.	.	.	.	.	.	.
6.	.	.	.	.	.	.	$f\sharp_{1,2}$	.	.	.	.	.	.
5.	.	.	.	.	.	.	.	.	.	.	.	\$b_{1,2,3}$	.
4.	.	.	.	.	\$e_{1,2,3}$	.	.	.	.	.	.	.	.
3.	.	.	.	.	.	.	.	.	.	\$a_{1,2,3}$	.	.	.
2.	.	.	\$d_{1,2,3}$	.	.	.	.	.	.	.	.	.	.
1.	.	.	.	.	.	.	.	\$g_{1,2,3}$	.	.	.	.	.
12.	\c_3	.	.	.	.	.	.	.	.	.	.	.	\c_3
11.	.	.	.	.	.	\f_3	.	.	.	.	.	.	.
10.	.	.	.	.	.	.	.	.	.	.	\$b\flat_2$	.	.
9.	.	.	.	\$e\flat_2$	.	.	.	.	.	.	.	.	.
8.	.	.	.	.	.	.	.	.	\$a\flat_{2,3}$	.	.	.	.
7.	.	\$c\sharp_{2,3}$	.	.	.	.	.	.	.	.	.	.	.
6.	.	.	.	.	.	.	\$f\sharp_2$,	.	.	.	.	.	.
5.	.	.	.	.	.	.	.	.	.	.	.	\\$b_{2,3}$	.
4.	.	.	.	.	\\e_3	.	.	.	.	.	.	.	.
3.	.	.	.	.	.	.	.	.	.	\\a_3	.	.	.
2.													
1.	.	.	.	.	.	.	.	\\g_3	.	.	.	.	.
12.	\\c_2	.	.	.	.	.	.	.	.	.	.	.	\\c_2
11.	.	.	.	.	.	\\f_3	.	.	.	.	.	.	.
10.	.	.	.	.	.	.	.	.	.	.	\\$b\flat_2$	.	.
9.	.	.	.	\\$e\flat_2$	.	.	.	.	.	.	.	.	.
8.	.	.	.	.	.	.	.	.	\\$a\flat_3$	.	.	.	.
7.	.	\\$c\sharp_3$	.	.	.	.	.	.	.	.	.	.	.

Application of Principle of Symmetrical Arrangement to a "Generalized Key-board" for Regular Systems.

A key-board has been constructed, on the principle of "symmetrical arrangement," in the following manner :—

The octave is taken $=6$ inches horizontally (in ordinary key-boards the octave is $6\frac{1}{2}$ inches). This is divided into 12 spaces, each $\frac{1}{2}$ inch broad. These are called the 12 principal divisions of the octave. A horizontal line gives the positions of an E. T. series where it crosses them all.

The keys are then placed at vertical and horizontal distances from the E. T. line corresponding to their departures, on the supposition that the arrangement is positive.

The departure of 12 fifths up corresponds to a horizontal displacement of 3 inches from the player, and a vertical displacement of 1 inch up.

These displacements are divided equally among the fifths to which they may be regarded as due, *i. e.* the displacement of *g* with respect to *c* is $\frac{1}{4}$ inch back and $\frac{1}{12}$ inch up ; so of *d* with respect to *g*, of *a* with respect to *d*, and so on.

Although only 3 inches of each key are thus exposed on a plan, yet the keys are all made to overhang $\frac{1}{2}$ inch, and thus the tangible length of each key is $3\frac{1}{2}$ inches.

The accompanying figure (p. 404) shows a small portion of the key-board, on a scale of half the real size.

The keys are each $\frac{3}{8}$ inch broad, and their centres are $\frac{1}{2}$ inch apart. There is thus $\frac{1}{8}$ inch free between the adjacent surfaces of each pair of keys, and $\frac{5}{8}$ inch altogether between the two keys which rise on each side of any given key. This is of importance ; *e.g.*, in the chord *c*–*e*–*g*–*c*, taken with the right hand, the first finger has to reach *e* between *e*♭–*f* and under the overhanging *e*.

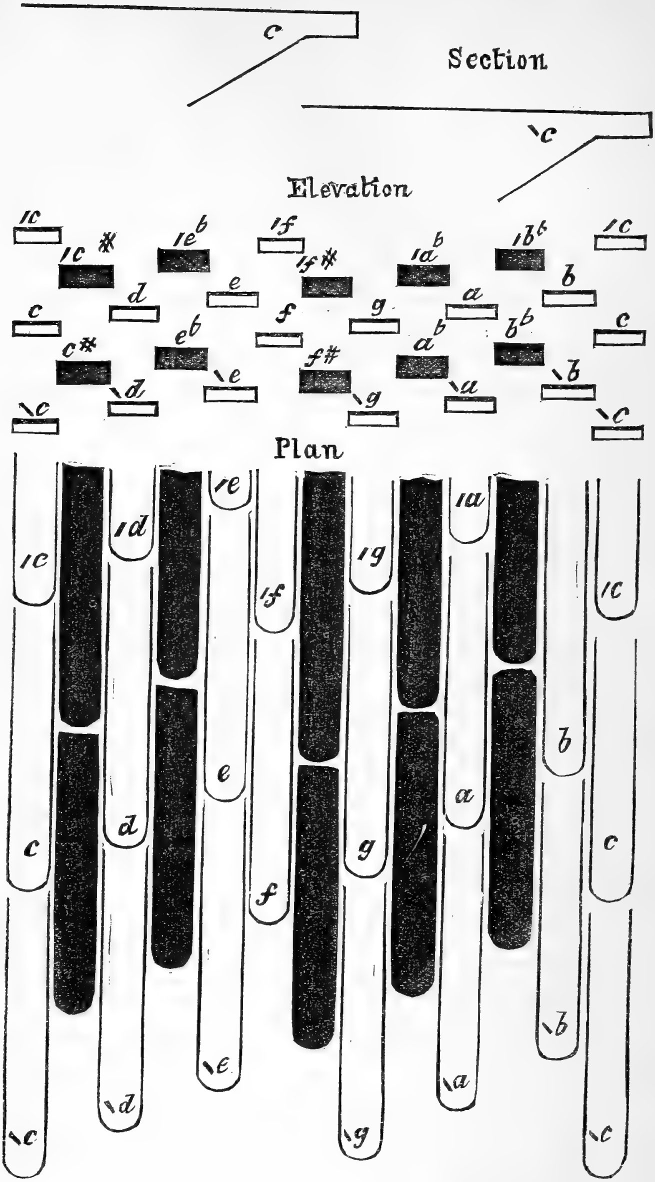
The keys in the five principal divisions which have "accidental" names (e.g. *c*♯ or *d*♭) are black, the rest white.

There are seven keys in each principal division; the seven *c*'s are marked from *c* to ///*c*, the unmarked *c* being in the middle. Thus there are 84 keys in each octave. The key-board controls an harmonium which contains the system of 53.

Application of the Positive System of Perfect Thirds to the "Generalized Key-board" (Helmholtz's system, just intonation).

If the thirds, such as *c*–*e*, are made perfect, and the fifths flat by ·00244, a quantity which escapes the ear, we have the system here mentioned. Helmholtz makes a mistake in describing it ('Die Lehre von den Tonempfindungen,' ed. 3, p. 495) ; he supposes that the fifths are sharp instead of flat by the above interval; it is easy to see from the context that this is a mistake.

The notation of positive systems is applicable without specialization.



Application of the Notation of Positive Systems to the System of 53.

The notation introduced for positive systems is susceptible of various accessory rules, according to the system it is attached to. In the harmonium to which the above-mentioned key-board belongs the system

of 53 is adopted. It is required to find rules of identification for passing from one principal division of the octave to another.

Rule.—In the system of 53 the notation of positive systems becomes subject to the following identifications:—

If two notes in adjoining principal divisions (e.g. *c* and *c*♯) be so situated as to admit of identification (*e.g.* a high *c* and a low *c*♯), they will be the same if the sum of the elevation- and depression-marks $=4$; unless the lower of the two divisions is black (accidental), then the sum of the marks of identical notes $=5$.

This can only be proved by enumeration of a case in each pair of divisions. This enumeration is made in the writer's original paper. It is founded on the following principles:—

Noting that the 5-fifths semitone is 4 units (scheme following Th. i.), we see that *c*–*c*♯ is 4 units, whence ////*c*–*c*♯, ///*c*–*c*♯, //*c*–*c*♯ .... are identities; or, again, *c*♯–*d* is 4 units, and ////*c*♯–*d*, ///*c*–*d* .... are identities.

Application of the System of 53 *to the "Generalized Key-board."*

An harmonium has been constructed which is arranged as follows:—

The note *c* is taken as the first note of the series, and receives the characteristic number 1. Then *c* is 4, and the remaining numbers can be assigned by the rules for the identifications in the system of 53 given above.

A number of notes at the top of the key-board are thus identical with corresponding notes in the adjacent principal divisions on the right at the bottom, *e.g.* //*c*=*b*=*c*♯. These permit the infinite freedom of modulation which is the characteristic of cyclical systems; for in moving upwards on the key-board we can, on arriving near the top, change the hands on to identical notes near the bottom, and so proceed further in the same direction, and *vice versâ*.

It is to be noted that, in positive systems, displacement upwards or downwards on the key-board takes place most readily by modulation between related major and minor keys—not, as has been commonly assumed, only by modulation round the circles of fifths. In negative systems, on the contrary, displacements take place only by modulations of the latter type.

Application of the System of 118 *to the "Generalized Key-board."*

The 5-fifths semitone is here 9 units, and the 7-fifths semitone is 11 units. The major tone (2-fifths tone) is consequently 20, and the minor tone (10-fifths tone) is 18. Hence the notes in the successive principal divisions are alternately odd and even, and the identifications lie in alternate columns. These are not here further investigated, as no practical use has been made of the system.

If $c=1$, $c\sharp=10$, $/c\sharp=12$, $d=21$, ...

It would be possible to construct a key-board on the principles already explained, which would give complete control over the notes of the system of 118. A portion of such a key-board would be practically indistinguishable from one tuned to the positive system of perfect thirds, as the error of the thirds of the system of 118 is too small to be perceived by the ear.

Application of the Negative System of Perfect Thirds (Mean-Tone System) to the "Generalized Key-board."

If the thirds, such as c–e, are made perfect, and the fifths ·05376 flat, we have the mean-tone system. The forms of scales and chords in negative systems are different from those in positive systems. The scales are very easy to play, and the chords also. It is expected that this application may prove of practical importance.

Following the scale of unmarked naturals on the plan, we can realize the nature of the fingering. It is the same as that of the Pythagorean scale with the system of perfect fifths. The tones are all 2-fifths tones, and the semitones both 5-fifths semitones.

Application of the Negative System of 31 *to the "Generalized Key-board."*

The fifths are a little better than in the last case, viz. ·05181 flat; the thirds ·00783 sharp. The only difference in the employment of the system is that the arrangement is cyclical. The tones all consist of five units, semitones of three.

The Investigation of Cycles of the Higher Orders—the new Cycle of 643 *and others.*

The system of 301 is of interest, as combining the properties of a tolerably good positive cyclical system with the representation of intervals accurately to three places by means of logarithms. This system has been lately used, in particular by Mr. Ellis, for approximate calculations. It appears to be of some interest to investigate generally what systems of higher orders do represent either of the systems with perfect thirds, and with what degree of accuracy they do so.

First, with respect to *positive systems*. If a system n of the rth order be a close approximation to the system of perfect thirds, then will $-8\frac{r}{n}$ (the departure of its third) approximate in value to $-\cdot13686$; or

$$\frac{r}{n}=\frac{\cdot13686}{8}=\frac{1}{58\cdot4526}\text{ nearly,}$$

or

$$n=r\;58\cdot4526\text{ nearly.}$$

Now, when $r=2$ we have the system of 118, which affords the closest approximation to what is required of any cyclical system known hitherto, the error of its third being ·00127.

Referring to Th. v., it is easy to see that no other even system of an order much below the 24th can afford a better approximation; for the number 118 differs from the value given by the above condition by little more than unity. Its multiple is always of the right order (Th. v.); there can therefore be no other system of the right order within 12 digits of the multiple either way, and the deviation of the value given by the condition cannot amount to 12 digits till near the 24th order; we therefore confine ourselves to systems of uneven orders.

Casting out 12's from 58·4526, we can take the remainder as 10·45 for the purposes of the search:—

r.	r. 10 . 45.	Remainder, casting out 12's.	Remainder required for order r (Th. iii.).
3	31·35	7·35	3
5	52·25	4·25	1
7	73·15	1·15	11
9	94·05	10·05	9
11	114·95	6·95	7

The coincidence at the 11th order is the closest so far; and it is easy to see, by considerations analogous to those above, that no subsequent system can afford another till a much higher order is reached.

For the 11th order, then, we have

$$11 \times 58{\cdot}4526 = 642{\cdot}9786\,;$$

and 643 is a system of the 11th order, as shown by its giving remainder 7 on dividing by 12 (Th. iii.).

Calculating the third of this system $\left(\frac{8\,.\,11}{633} = \text{dep.}\right)$, and taking seven places, we have:—

$$\begin{aligned} \text{Departure of perfect third} &= -{\cdot}1368629 \\ \text{Departure of third of 643} &= -{\cdot}1368585 \\ \hline \text{Error} &= \;\;{\cdot}0000044 \text{ sharp.} \end{aligned}$$

To five places both thirds are represented by $-{\cdot}13686$.

The intervals of this system will furnish us with simple numerical ratios, which represent with great accuracy the intervals of the perfect system.

We have (see the section on the number of units in any interval)—

$$\begin{aligned} \text{7-fifths semitone} &= 60 \text{ units,} \\ \text{5-fifths semitone} &= 49 \text{ units;} \end{aligned}$$

whence we can deduce the remaining intervals. These values of the semitones suggest the following curious derivation of this system :—

Referring to the Table of characteristic numbers, we notice that the errors of the thirds of the systems of 53 and 65 are nearly equal and opposite.

The system of 53 is derived on the assumption that the interval ratio of the semitones is $\frac{4}{5}$ (Th. i. Cor.), and that of 65 on the assumption $\frac{5}{6}$ for the same ratio; taking, then, an intermediate ratio, $\frac{9}{11}$, we get the system of 118, which has very good thirds.

But if we take an intermediate ratio in the following manner, we get the new system of 643 :—

Reducing the fractions $\frac{4}{5}$, $\frac{5}{6}$ to a common denominator, we have $\frac{24}{30}$, $\frac{25}{30}$, or doubling, $\frac{48}{60}$, $\frac{50}{60}$; and if we take the intermediate ratio $\frac{49}{60}$, we get the system of 643, by the formula $5x+7y=\dot{n}$, derived from Th. *a* of Regular Systems.

The systems of the fifth order are not particularly good; the best is 289, then 301. They derive their interest from the logarithmic properties of 301.

Negative Systems.—The condition for the excellence of the thirds of negative systems is that

$$4\frac{r}{n} = -\cdot 13686 \text{ nearly,}$$

or

$$\frac{r}{n} = -29\cdot 2263 \text{ nearly.}$$

Searching as before, we find for order -7,

$$7 \times 29\cdot 2263 = 204\cdot 5841;$$

and 205 is a system of order -7.

Comparing thirds,

Departure of perfect third	=	$-\cdot 1368629$
Departure of third of 205	=	$-\cdot 1369002$
Error	=	$\cdot 0000373$ flat.

The following is a *résumé* of the properties of these higher systems :—

System.	Order.	$\Delta = 12\delta$.	Error of fifth.	Error of third.
289	5	$\cdot 20761$	$-\cdot 00225$	$-\cdot 00155$
643	11	$\cdot 20529$	$-\cdot 00244$	$+\cdot 0000044$
301	5	$\cdot 19934$	$-\cdot 00294$	$+\cdot 00397$
205	-7	$-\cdot 41070$	$-\cdot 05377$	$-\cdot 000037$

"Experiments on the Brain of Monkeys.—No. I." By David Ferrier, M.A., M.D., M.R.C.P., Professor of Forensic Medicine, King's College, London. Communicated by Dr. J. B. Sanderson, F.R.S.

The facts recorded in this paper are the results obtained by electrical stimulation of the brain of monkeys, after the method described by the author in the West Riding Lunatic Asylum Medical Reports, vol. iii. 1873. They formed part of a paper "On the Localization of Function in the Brain," read before the Royal Society on March 5, 1874*. This memoir also contained the results of other experiments on the brain of monkeys, chiefly relating to the effects of localized lesions of several parts of the hemispheres, with a view to determine the significance, as regards sensation and motion, of the phenomena caused by electrical irritation. These experiments are not here recorded, but are reserved for comparison with the results of a more extended reinvestigation of a similar nature, on which the author has been for some time engaged, and which will shortly be laid before the Society.

In order to avoid unnecessary detail, and in order to place the results together for the purposes of comparison, the animals experimented on are described, the dates of experiment given, and numbers assigned to them, so that they may all be brought into relation with each other :—

Experiments on Monkeys (Macaques).

I.	Left hemisphere	....	June 14, 1873.
II.	Right ,,	....	June 18, ,,
III.	Left ,,	....	June 23, ,,
IV.	Left ,,	....	June 25, ,,
V.	Left ,,	....	June 27, ,,
VI.	Right ,,	....	July 4, ,,
VII.	Left ,,	....	July 16, ,,
VIII.	Left ,,	....	July 22, ,,
IX.	Right ,,	....	July 25, ,,
X.	Right ,,	....	Aug. 1, ,,
XI.	Left ,,	....	Aug. 8, ,,
XII.	Right ,,	....	Aug. 23, ,,
XIII.	Right ,,	....	Sept. 5, ,,

The circles marked on the woodcuts indicate the regions stimulation of which is followed by the same results. Several applications of the electrodes (which do not cover a larger diameter than a quarter of an inch) in or near the same region are necessary to mark off the area. To exactly define it is hardly possible, as the areas overlap each other, so that a complex set of movements may be caused by the conjoint stimula-

* See Proceedings, vol. xxii. p. 229.

tion of two centres which individually are capable of differentiation. This is particularly liable to occur if the currents are too strong. The areas drawn on the woodcuts are therefore more or less indefinite as to their boundaries. Their centres indicate more precisely the points of exact localization.

Besides describing the results of stimulation by reference to the figures, I have indicated the position of the electrodes, as far as possible, in relation to the individual convolutions, so that comparison may be made with those of the human brain.

For this reason the results are classified, and not related in the order in which they were obtained during the course of experiment.

Fig. 1.

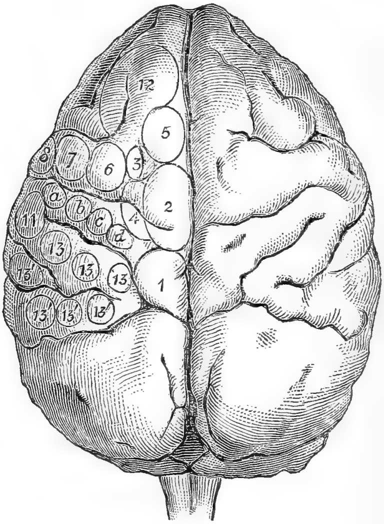
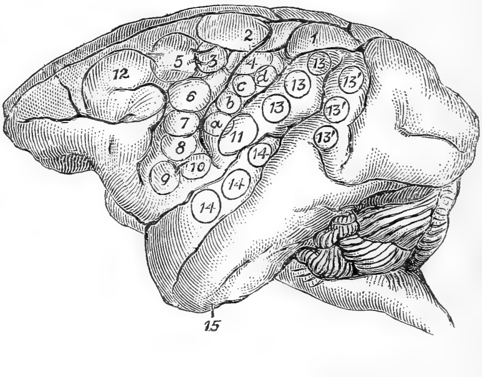


Fig. 2.



Circle (1), figs. 1 & 2, corresponding to the postero-parietal lobule, or superior extremity of the ascending parietal convolution. The region is embraced between the parieto-occipital fissure and a short perpendicular sulcus at right angles to the median fissure.

Results of stimulation:—

I. Not explored.

II. The left foot is flexed on the leg, and the toes are spread out and extended.

III. The right thigh is slightly flexed on the pelvis, the leg is extended, the foot flexed on the leg, and the toes are extended.

This result was obtained by stimulation of the posterior margin of the circle. At other points the advance of the whole limb was not so distinct, but the flexion of the foot and extension of the toes was very marked.

IV. The right leg is advanced, the foot flexed, and the toes extended.

In this case some movements of the arm were also made, but not of a constant nature, and were therefore regarded as accidental complications.

V. The right thigh is flexed on the pelvis, the leg extended, the foot flexed on the ankle, and the toes extended.

VI. Flexion of the left thigh on the pelvis, extension of the leg, flexion of the foot on the ankle, extension and spreading out of the toes.

VII. The right hind leg is advanced as in walking, the foot flexed on the ankle, and the toes extended.

In this case slight adduction of the foot was observed on stimulation, just posterior to the lower end of the short perpendicular sulcus already referred to.

VIII. Extension of the toes of the right foot, and flexion of the foot on the ankle.

In this case also a tendency to adduction of the leg and foot was observed.

IX. Flexion of left thigh on pelvis, extension of the toes, and flexion of the floot. (One observation only.)

X. Not explored.

XI. Extension of the toes of the right foot, and flexion of the foot on the ankle.

XII. Not explored.

XIII. Not explored.

The general result of stimulation of this region is to show that it is a centre for the movements of the hind leg, and apparently those concerned in walking.

It will be observed that in some cases the movement is only partially carried out. This is frequently the case, and, as will be seen in the subsequent details, a movement, at first of limited extent, gradually merges into a more complex one, involving numerous muscles.

Circle (2), figs. 1 & 2. This embraces the upper extremity of the ascending frontal convolution, and also stretches across the fissure of Rolando, so as to include the anterior division of the upper extremity of the ascending parietal convolution.

In the description reference is made to an anterior and posterior division, the boundary between the two being the fissure of Rolando. This is done because, though on analysis movements excited by stimulation of both are essentially the same, they are less distinctly brought out by stimulation of the posterior division alone, and are apt to merge into those resulting from excitation of circle (1).

Results of stimulation of the posterior division :—

I. Not explored.

II. Not explored.

III. Not explored.

IV. Not explored.

V. In this case there were movements of the right hind leg and foot, consisting in extension and then flexion and abduction of the thigh. Nothing more definite could be ascertained.

VI. In this case also there was first extension of the left thigh, then abduction or rotation outwards, while the foot was rotated inwards, the toes being extended and spread out.

VII. Not explored.

VIII. Rotation outwards of the right thigh, and rotation inwards of the leg and foot.

IX. Not explored.

X. Not explored.

XI. Similar to VIII.

XII. Not explored.

XIII. Not explored.

Results of stimulation of the anterior division (or upper part of the ascending frontal) :—

I. Not explored.

II. At first the left thigh was extended, and the leg and foot slightly rotated inwards, and then, on continuation of the electrization, the thigh was flexed on the pelvis and rotated outwards, while the leg and foot were rotated inwards, the toes being spread out in a

semiflexed condition, and pointing to the middle line of the body.

The combination of actions is just such as when a monkey scratches its abdomen with its hind leg.

The muscles of the trunk participated in the movements, so that the body was twisted to the opposite side.

III. The action in this case was in all respects similar to that recorded in II.

IV. In this case epileptic or choreic convulsions, which readily occurred on the slightest stimulation, rendered analysis of the movements impossible.

V. In this case the action described in II. was very distinct, viz. the rotation outwards of the thigh, the rotation inwards of the leg and foot, and the grasping portion of the toes pointing towards the middle line.

VI. As in I. and V., the thigh on the opposite side (left) was flexed and rotated outwards, the leg and foot inwards, while the toes were spread out.

VII. In this case also the movements were very distinct, consisting in rapid combined muscular action, bringing the foot and toes inward as if to scratch the body.

VIII. The results in all respects the same as VII.

IX. Similar action, viz. rotation outwards of the thigh, inwards of the leg, and the foot brought up with a sort of grasping movement of the toes to the middle line of the trunk.

X. Not explored.

XI. Action in all respects as described in IX.

XII. Not explored.

XIII. Not explored.

Circle 3, figs. 1 & 2, corresponding to the situation of a parallel sulcus in the upper part of the ascending frontal convolution.

It may be taken as included in the previous one, but is marked separately on account of being also a centre for the tail.

Results of stimulation :—

I. Twisting of the trunk to the left, along with some not well-defined movements of the right leg and tail.

II. In this case the same action was observed as resulting from stimulation of circle 2, viz. flexion of thigh, rotation outwards, leg and foot rotated inwards, with the toes stretched out, semiflexed towards the middle line of the body. Movement of the tail was not noted.

III. Similar action. Tail not noted.

IV. Not explored.

V. Similar action of the leg, but at the same time the right arm was retracted. (This action of the arm will receive explanation below.) The tail was not observed.

VI. Not explored.

VII. Movements of the right hind leg as before, and also of the tail. The movement of the tail was not noted as being of a definite character, nor whether it was moved to the right or left.

VIII. The same as VII.

IX. Also the same as VII.

X. Not explored.

XI. Gave the results described under VII.

XII. Not explored.

XIII. Not explored.

The general result of these observations, all agreeing with each other in essential respects, is to show that circle 2 is a centre for the hind leg of a different character from circle 1, being more concerned in the action of the hind leg as an organ of prehension or climbing, instead of being an organ of simple progression. The subdivision circle 3 seems to be a centre for the tail. As I have not had an opportunity of experimenting on monkeys with prehensile tails, I am unable to indicate further the special action of this centre in regard to it.

Circle 4, figs. 1 & 2, embracing a portion both of the ascending frontal and of the ascending parietal convolution, extending from the lower border of the parallel sulcus, already alluded to, to the anterior boundary of circle 1.

I. Not explored definitely.

II. The left humerus is adducted, the hand pronated, the whole arm straightened out and drawn backwards.

The action is such as is attributed to the latissimus dorsi, viz. a sort of swimming-action of the arm, with the palm of the hand directed backwards.

III. A similar extension and retraction backwards of the right arm.

IV. Retraction of the right arm, with the hand as already described.

V. Action as before of the latissimus dorsi, but at the same time also the right hind leg is acted on as if by stimulation of circle 1. In this case also the hand was firmly clenched.

Here it must be noted that the centres which cause the above-described movements of the hind leg, as well as those afterwards to be described which cause clenching of the fist, had been under stimulation previous to the exploration of this region. Hence the result is to be looked upon as the combined action of all three centres. This is the real difficulty experienced in analysis of the complex movements of the limbs, there being always a tendency to have complications arising from the irritable condition which continues in the regions which have been under experimentation. This, along with the tendency to convulsive spasms of a choreic or epileptiform nature, lasting for minutes after the cessation of stimulation, renders it frequently excessively difficult to draw accurate conclusions. The results described have always been those arrived at after as complete exclusion as possible of these adverse conditions.

VI. Retraction and straightening out of the left arm, as already described.

VII. Noted as action of the latissimus dorsi, this being regarded as the chief cause of the movement.

VIII. In this case the shoulder was first elevated, the humerus adducted, the wrist and hand fully extended, and the whole arm straightened and drawn backward in the manner already described in II.

IX. Left arm adducted, and then extended and retracted.

X. A similar result.

XI. Not explored.

XII. Not explored.

XIII. Action of the latissimus dorsi as already described. The condition of the hand is not noted.

The results of stimulation, therefore, of this region agree with each other.

I have observed frequently that only very partial action was occasionally obtained at some points in this circle; sometimes only an appearance of adduction of the arm. The complete action, however, appears to be such as I have described as resulting from stimulation of the centre of this circle.

Circle (5), figs. 1 & 2, corresponding to the posterior third of the superior frontal convolution.

I. The results in this case were not very definite. They consisted in a complication of the movements of the leg, already described as resulting from circle (2), along with an extension forwards of the right arm.

II. Here also extension forwards of the left arm and also of the leg.
It is to be noted that the leg-centres had been already under stimulation previous to exploration of this centre.

III. Extension forwards of the right arm and hand.

IV. The results were not very definite, as the animal was continually being thrown into convulsive spasms on application of the electrodes.

V. The right arm and hand are extended forwards, as if to touch or reach something in front.

VI. Not explored.

VII. Rapid extension forwards of the whole right arm and hand.

VIII. Similar action.

IX. The left arm is outstretched, as if to touch some object in front.

X. Not explored.

XI. Similar result.

XII. Not explored.

XIII. Indefinite result, but apparently resembling IX.

Excluding complication due to conjoint irritation of the leg-centres, the results indicate a centre for a definite forward extension of the arm.

Ascending parietal convolution (*a*), (*b*), (*c*), (*d*), figs. 1 & 2. This is marked by the letters *a*, *b*, *c*, and *d*, indicating circles extending from the centre of the convolution to the fissure of Rolando and intra-parietal sulcus respectively.

Owing to the fact that many individual variations exist in the results obtained, they are detailed with more fulness in each case in the order of the experimentation, care being taken to compare accurately corresponding regions. The divergences are in a great measure to be explained by the fact that the movements caused involve the conjoint action of the flexor and extensor muscles of the wrist and fingers, and hence movements of an apparently opposite character seem to result from stimulation of the same point. A differentiation of centres for extension and centres for flexion could not be made out.

I. *a*. Flexion of the fingers of the right hand, abduction of the thumb, and slight rotation outwards of the wrist.

b. A similar action, but at the same time the humerus is adducted.

c. Similar to *b*.

d. Pronation of hand and flexion of the thumb and fingers.

Longer stimulation gave rise to clonic spasms of the wrist and fingers, continuing several seconds.

II. *d*. Sudden and quick extension of the wrist, and slightly of the fingers.

Continued in a choreic manner after stimulation.

c. Same result as *d.*

b. Extension of the fingers, but more especially of the thumb.

a. Thumb only is extended.

Spasmodic jerking of the thumb continued several seconds after withdrawal of the electrodes.

III. *a.* Clenching of the fist. The movement began with the thumb, which was first adducted.

b. As before, clenching of the fist; at the same time the extensors of the wrist and fingers were seen to be contracted.

c. Momentary application of the electrodes caused adduction of the thumb, followed on longer stimulation by clenching of the whole fist and slight pronation of the hand.

After several other regions had been explored, these points were again stimulated in succession.

The second result at point *d* was extension of the thumb and fingers instead of flexion.

A second stimulation of point *b* caused, first, adduction of all the fingers, then extension of the wrist and flexion of the distal phalanges, the proximal phalanges not being flexed till the wrist was fully extended.

The repeated application of the electrodes was followed in this animal by an epileptiform fit, affecting both sides of the body and lasting for three minutes.

IV. *a.* Abduction of the thumb.

At the same time the right angle of the mouth was retracted, owing, as will be seen, to the proximity of the centre for the platysma.

b. At first extension of the thumb, then, on longer irritation, extension of the wrist and flexion of the fingers.

c. Thumb adducted, and then firm clenching of the fist.

d. Clenching of the fist and pronation of the hand.

V. *a.* One application of the electrodes caused extension of the thumb; another caused adduction of the thumb and clenching of the fist, with extension of the wrist.

b. Clenching of the fist and extension of the wrist, as before.

c. Clenching of the fist as before, with complete pronation of the hand.

d. Clenching of the fist repeated, but complicated with the action of the latissimus dorsi and backward extension of the arm.

The reason of this is the proximity of the point *d* to circle 4.

VI. *a, b, c, d.* A similar result in all, viz. flexion of the fingers and extension of the carpus.

VII. *a.* Clenching of the fist.

The whole convolution was not explored, on account of constant repetition of choreic-like spasms.

VIII. *a.* Flexion of the fingers, passing into firm closure of the whole fist.

This action, slowly performed, began with the thumb, which became adducted, then the index-finger, the fingers in succession becoming adducted and then flexed till the fist became firmly closed.

b. Flexion and adduction of the thumb alone.

c. First adduction and flexion of the thumb, then firm closure of the fist and pronation of the arm.

IX. *a, b, c, d.* Slight touch causes adduction of the thumb, followed on longer stimulation by flexion of the fingers and complete closure of the fist.

Stimulation close to the fissure of Rolando caused the same movements, and also very decided extension of the wrist when the fist was completely closed.

X. Not explored.

XI. Results essentially similar to IX.

XII. Not explored.

XIII. Clenching of the fist as in former experiments.

The variations described in the movements resulting from stimulation of the ascending parietal convolution are apparently all referable to different aspects of combined muscular contractions, which in their completest action serve to cause closure of the fist or the grasping-action of the hand. Centres for the extensors and flexors, or for the flexors and extensors, of the individual digits could not be definitely made out.

Owing to the proximity of the centre for the platysma (circle 11) at the lower end of the ascending parietal, very frequently, along with firm closure of the fist, there was decided retraction of the angle of the mouth on the same side.

Ascending frontal convolution :—

Circle 6, figs. 1 & 2. The position of this is on a level with the posterior extremity of the middle frontal convolution.

I. Supination of the hand and flexion of the forearm on the humerus, the hand being also more or less clenched. The action is such as may be attributed to the biceps, along with action of the flexors of the fingers.

Long-continued stimulation brings the hand up to the mouth, and at the same time the angle of the mouth is retracted and elevated.

This action of the mouth will be explained by reference to the action of the centre immediately below it.

II. Supination and flexion of the forearm and hand.

III. A similar result.

IV. Flexion of the forearm, clenching and supination of the hand.

V. Shoulder raised, forearm firmly flexed, hand clenched and supinated. The hand ultimately raised to the mouth, the angle of which is retracted and elevated.

VI. Flexion, with slight supination, of the forearm and hand.

VII. Flexion and supination of the right forearm and hand, accompanied with clenching of the fist when the stimulation was applied near the fissure of Rolando.

VIII. Apparent action of the biceps as before.

IX. Flexion and supination of the forearm and hand.

In this case it was found very decidedly that stimulation close to the fissure-of-Rolando side of the convolution caused the action of the biceps to be associated with clenching of the fist. Towards the lower margin of the circle the same movements were associated with retraction of the angle of the mouth.

X. Not explored.

XI. Same results as IX. exactly.

XII. Not explored.

XIII. Results as in other cases, viz. flexion and supination of forearm and hand, with clenching of the fist.

These uniform results point very clearly to this as the centre for the biceps and muscles concerned in bringing the hand up to the mouth.

Circle (7), figs. 1 & 2. Still in the ascending frontal convolution, in position immediately below the centre for the biceps.

I. Retraction and elevation of the right angle of the mouth.

II. Retraction (with elevation) of left angle of the mouth. Occasionally in stimulation the action was conjoined with that of the biceps.

III. Not explored.

IV. Not explored.

V. Spasm of the right angle of the mouth and of the cheek-pouch.

VI. Not explored.

VII. Elevation of right angle of mouth.

VIII. Same result as VII.

IX. Angle of the mouth raised and retracted, along with action of the biceps and flexors of the fingers.

X. A similar result. In this case, after several other parts had been under exploration, excitation of this region gave rise to a species

of epileptic fit, beginning in the left angle of the mouth, next proceeding to the left arm and hand, and, lastly, affecting the left leg and tail. The spasms next attacked the right angle of the mouth, the right arm, and the right leg in succession.

The fit lasted several minutes. The pupils were not dilated, nor did the animal apparently lose consciousness completely.

XI. Previous results confirmed.

XII. Not explored.

XIII. Contraction of left angle of mouth.

These results indicate that this is a centre for the muscles acting on the angle of the mouth, and apparently of the zygomatics.

Circle (8), figs. 1 & 2. Lower down in the same ascending frontal convolution.

I. The action is similar to that resulting from stimulation of the former centre, but seems especially to cause elevation of the lip and ala of the nose on the right side.

II. At the anterior part of the circle the left angle of the mouth is drawn upwards and backwards (zygomatici). At the posterior and lower margin of the circle the action is combined with that of the depressor anguli oris, so as to expose the canine teeth.

III. Not explored.

IV. Not explored.

V. Elevation of the upper lip (right side) and right side of nose.

Stimulation was followed by prolonged choreic-like twitching of the right angle of the mouth and ala of nose.

VI. Not explored.

VII. Elevation of right side of upper lip and ala of nose, along with depression of the lower lip.

VIII. Combined action of the elevator of the upper lip and ala of the nose and of the depressor anguli iris, so as to expose the canine teeth.

IX. Elevation of the upper lip and depression of the lower, so as to cause divergence of the lips and expose the teeth.

X. A similar result, but not so distinct, as the occurrence of choreic spasms interfered with successful observation.

XI. Results as in IX.

XII. Not explored.

XIII. At lower part of circle the depressor anguli oris is thrown into action; at the upper part the angle of the mouth is elevated.

These results are sufficiently uniform to indicate a centre for the facial muscles concerned in the production of that expressional action so fre-

quently exhibited by monkeys under the influence of fear or anger, viz. the exposure of the canine teeth.

Circles (9) and (10), fig. 2, corresponding in situation to the lower part of the ascending frontal convolution, or posterior part of the inferior frontal convolution, above the lower end of the fissure of Sylvius (Broca's convolution).

I. (9). The lips pout, mouth gradually opens, and the tongue is protruded.

(10). Action similar as to the mouth, but the tongue is retracted. Longer stimulation caused movements of the mouth and tongue, as in mastication.

II. (9). Mouth opened and tongue protruded.

(10). Tongue retracted.

Movements of mastication made by continued stimulation.

III. Same results as in I. and II.

IV. Not explored.

V. (9), as in former cases, causes opening of the mouth and protrusion of the tongue.

(10) causes retraction of the tongue.

Movements of mastication also caused on longer stimulation.

VI. Not explored.

VII. (9). Mouth opened and tongue protruded.

(10). Same result, but tongue apparently retracted.

VIII. (9). Opening of the mouth and protrusion of the tongue.

(10). Same result, with retraction of the tongue, followed on continuous stimulation with opening and shutting of the mouth, and alternate protrusion and retraction of the tongue.

IX. Similar results to VIII.

X. Not explored.

XI. Results of VIII. confirmed.

XII. Not explored.

XIII. Movements of the mouth and tongue, but not of any very definite character, the animal being in a state of exhaustion, and the excitability of the brain very weak.

These results point very definitely to a centre for the movements of the mouth and tongue, the muscles concerned in mastication and also in articulation. Its position is significant, as being the homologue in man of that region which is the seat of lesion in the disease known as aphasia, described as the posterior extremity of the lower frontal convolution. (In aphasia the lesion is generally on the left side, but the bilateral movements are seen by the experiments to be induced from both right and left sides.)

Circle (11), figs. 1 & 2, corresponding to the lower termination of the ascending parietal convolution and region of the inferior termination of the intraparietal sulcus (the conjoint extremities of the ascending parietal and angular gyrus).

I. Retraction of the right angle of the mouth, apparently the platysma thrown into action.

The effect was kept up after stimulation in a spasmodic manner.

II. Not explored.

III. Right angle of the mouth retracted. The head becomes drawn to the right by powerful contraction of the platysma.

IV. Not explored.

V. Retraction of the right angle of the mouth.

VI. Not explored.

VII. Not explored.

VIII. Retraction, with some appearance of elevation of the right angle of the mouth, along with powerful contraction of the subcutaneous muscles on the right side of the neck, evidently the platysma.

IX. Retraction of the left angle of the mouth. In this case the mouth was partially opened so as to expose the teeth.

X. Retraction of the left angle of the mouth.

This point was explored after several other regions had been under stimulation. The application of the electrodes to this point gave rise to spasms of a choreic or epileptiform nature, beginning in the left angle of the mouth, then affecting the left hand and arm, and lastly the left leg and tail. The choreic spasms then passed to the right angle of the mouth, the right hand and arm, and in a slight degree to the right leg and tail.

The fit lasted one or two minutes. It had not all the characters of a fully pronounced epileptic attack.

XI. Retraction of the right angle of the mouth. Recorded as action of the platysma.

XII. Not explored.

XIII. Strong contraction of the platysma, and retraction of the left angle of the mouth.

The results of stimulation of this centre agree with each other, and indicate a centre for the platysma. The frequent retraction of the angle of the mouth observed on causing clenching of the fist is explained by the proximity of the two centres to each other.

Island of Reil (central lobe), within fissure of Sylvius:—

Owing to the central lobe in the monkey being completely concealed within the lips of the fissure of Sylvius, mechanical injury and considerable hæmorrhage is necessarily caused in the attempt to expose it clearly. This is mentioned as a possible explanation of the negative results, but it is not sufficient to account for the apparent non-excitability of this region.

The island was exposed and experimented on in monkeys IX. and XIII.

IX. Electrization of the island of Reil gave no results.

Some movements of the mouth were caused during the introduction of the electrodes within the fissure, but were referred to stimulation of the mouth-centres in close proximity.

XIII. The result in this case was also negative.

To test this matter more fully, another monkey, not among those already numbered, was experimented on on December 10.

The lips of the fissure of Sylvius were carefully separated, without causing much injury or hæmorrhage. After the hæmorrhage had entirely ceased, the electrodes were applied directly to the surface of the central lobe.

No effect was observed.

After the animal had been allowed to rest for some time, it was then tested as to the excitability of the other centres. The hand, leg, and mouth could as usual be acted on by stimulation of their respective centres.

The electrodes, insulated up to the point, were again applied to the island of Reil.

No result was observed.

Stronger and continuous stimulation gave rise to choreic spasms of the angle of the mouth. This was attributed to diffusion of the current, owing to its being strengthened, and irritation of the proximate centres for the angle of the mouth.

Another application of the electrodes within the lower end of the fissure caused movements of the mouth and tongue. These also may have been due to conduction to the mouth-centres already described.

Beyond these, stimulation of the island of Reil yields negative results.

Circle (12), figs. 1 & 2, including the superior and middle frontal convolution from the antero-parietal sulcus (Huxley), sulcus præcentralis (Ecker), to the anterior extremity of the supero-frontal sulcus.

The results of stimulation of these convolutions were always so uniform, that the general result of experimentation in ten monkeys may be stated together. The results were :—

Elevation of the eyebrows and the upper eyelids, turning of the eyes and head to the opposite side, and great dilatation of both pupils.

Occasionally on stimulation of the centre for the forward extension of the hand this movement of the eyes and head was called into play.

Inferior frontal convolution (including all in advance of the sulcus præcentralis).

Stimulation of this region gave no results.

Antero-frontal region (including all in advance of the anterior extremity of the supero-frontal sulcus, and indicated sometimes by a slight sulcus at right angles to the median fissure) and orbital convolution.

These regions were subjected to stimulation in four cases, viz. I., V., VIII., and IX.

No results could be observed, either from the antero-frontal or orbital regions.

In a later experiment (December 2) on another monkey it was found that stimulation of the frontal part of the brain caused the eyes to move to the opposite side. This was found to be the case with irritation of both right and left hemispheres. The eyelids were not always opened, however, nor was dilatation of the pupils observed. Sometimes also the eyes moved upwards, instead of to the opposite side.

Irritation, therefore, of this region gives nothing definite as to their function.

Angular gyrus (*pli courbe*, Gratiolet).

Circle (13) and (13′), figs. 1 & 2. This is referred to as being composed of an ascending or anterior and a descending or posterior limb.

Results of stimulation :—

Ascending limb, circle (13).

I. Eyes directed upwards and to the right. Some oscillation of the right eyeball continued after the withdrawal of the electrodes.

On longer-continued stimulation the head is turned to the right, and the eyeballs to the right and slightly upwards.

II. Not explored.

III. Eyes directed upwards.
IV. Not explored.
V. Not explored.
VI. Not explored.
VII. Both eyes are directed to the right (whether there was any upward direction was not noted).
The pupils became contracted.
VIII. The eyes were directed to the right (notes do not mention as to whether any upward direction was observed). The pupil was thought to be slightly contracted. The eyelids during the stimulation had a tendency to close. The head also inclined to the right side.
IX. Both eyes directed upwards and to the left. Pupils contracted?
In this animal, which was allowed to remain quite conscious during stimulation, an experiment was made as to vision by holding before it a teaspoonful of milk, which it was eager to seize. In its attempt this point was stimulated, with the effect of causing confusion of vision and some difficulty in reaching the milk.
X. Both eyes turned to the left and slightly upwards. The pupils contract and the eyelids tend to close.
XI. Both eyes to the right and upwards. Pupils not observed.
XII. Both eyes to the left and upwards.
XIII. Both eyes to the left and upwards. The pupils contract.

Descending limb, circle (13′).

I. Eyes to the right and downwards. Head is inclined to the right side.
II., IV., V., VI. Not explored.
III. Eyes to the right and downwards.
VII. Both eyes directed to the right. Pupils contract.
VIII. Eyes down and to the right. Eyelids tend to close. Head directed slightly to the right side.
IX. Eyes directed down and to the left.
X. Eyes to the left and slightly downwards. The eyes half closed. Pupils contract.
XI. Eyes to the right and downwards. Pupils not observed.
XII. Same as XI.
XIII. Both eyes directed downwards and to the left. The pupils contracted.

These results are obtained on the *pli courbe* from the centre for the platysma (circle (11)) down to the termination of the descending limb in the *pli de passage* connecting it with the occipital lobe.

Experiments will be given subsequently as to the effects of destruction

of this region, and an attempt made to interpret the signification of these movements of the eyeballs.

Superior temporo-sphenoidal convolution, circles (14), fig. 2 (extending for about two thirds of its length from above downwards).

The results are only completely described after VII., as the ear was not particularly observed in the experiments going before. Results:—

I. Eyes opened and head turned to the right. Nothing observed as to the state of the pupils or ear.
II. Eyes open; eyeballs directed to the left, pupils dilate.
III. Eyes to the right, pupils dilate.
IV. Head and eyes quickly turn to the right. Pupils not observed.
V. Not explored.
VI. Not explored.
VII. Retraction (pricking) of the right ear, eyes widely opened, pupils dilated, and head and eyes turned rapidly to the right.
VIII. Retraction of right ear, head to the right, eyelids opened widely, eyes directed to the right with great dilatation of the pupils.
IX. Retraction of left ear, eyes opened widely, head quickly turned to the left. Pupils not observed.
X. Retraction of left ear, head and eyes turned to the left, and dilatation of the pupils.
XI. Retraction of right ear, eyes and head turned to the right, with dilatation of the pupils.
XII. A precisely similar result on left side.
XIII. Exactly same as XII.

The uniformity in the later experiments is complete. The results obtained are always quick and decided; they seem a combination of pricking of the ear, along with the effects described as resulting from stimulation of the frontal regions (circle (12)). Their significance will be alluded to subsequently.

The lower extremity of the same temporo-sphenoidal convolution gave no results in any of the animals in which this region was experimented on, viz. I., V., VIII., IX., X., and XIII.

Middle temporo-sphenoidal convolution (from the *pli de passage* downwards).

Nothing very definite was arrived at. In some the results were altogether negative; in others the following phenomena were noted, perhaps not altogether satisfactory as to their nature.

IX. On irritation of the lower end of the temporo-sphenoidal, just anterior to the lower temporo-sphenoidal convolution, a pursing

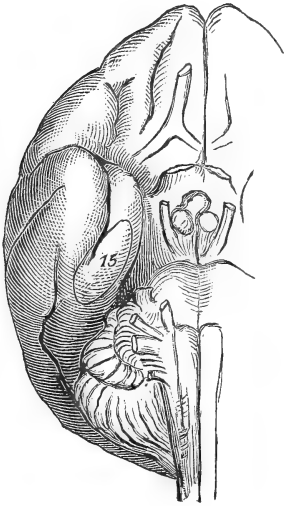
of the mouth, with some movements of the cheek-pouches and tongue, were observed.

X. In this case stimulation of the corresponding region caused some indistinct movements of the mouth and lips.

XIII. In this case there were some movements of the jaws; not of any decided character.

These are all the facts I have been able to gather from experimentation on this region, which is attended with some difficulty.

Fig. 3.



Lower temporo-sphenoidal convolution (inner aspect) and region of the uncinate convolution and occipito-temporal gyrus. Circle (15), figs. 2 & 3.

This region was reached and stimulated in the following cases with these results:—

VIII. Spasmodic contraction of the left lip and ala of the nose. The result was a sort of torsion or closure of the nostril, as when an irritant is applied to it. The action was on the same side, not crossed, as usual.

IX. Spasmodic torsion of the right lip and nostril, also on same side as stimulation.

X. Similar results, viz. an elevation of the right nostril and lip, so as to cause partial closure. In this case the phenomenon was observed on both sides, the right more especially, however.

XIII. Torsion of the right lip and nostril, as before.

In all these cases the phenomena were exactly alike. The fact of

their being on the same side as the stimulation, is explained by the origin of the olfactory tract from the *subiculum cornu ammonis,* and which passes on to the olfactory bulb without decussation.

The results plainly indicate a perception or subjective sensation of odours, and point to this as the central seat of the sense of smell.

Occipital lobes (superior and middle convolutions).

These were experimented on in I., III., V., VII., VIII., IX., X., XI., and XII.; also in another, not numbered, on November 21.

The results, except in the case of X., to be afterwards mentioned, were altogether negative as far as outward phenomena were concerned.

The negative results are not to be attributed to exhaustion of the excitability of the brain, for the other centres at the same time gave the usual results.

In the case of X. it was observed that stimulation of the inferior occipital convolution towards its inner aspect caused uneasy movements in the hind legs and tail, the head being turned to the left (opposite side) and backwards. Occasionally also a plaintive cry, as if from annoyance, was uttered. On cessation of the irritation the animal subsided into its dozing state.

Possibly the result may be attributed to conduction of the current to the tentorium or other part of the dura mater; but, owing to the difficulty of reaching this part of the brain, it is not easy to avoid all sources of fallacy.

Marginal convolution.

In the case of IX., the only one explored, it was found that irritation of the median aspects of the frontal and parietal convolutions caused movements of the head and limbs similar to those caused by stimulation of the centres on the outer aspect of the hemisphere.

Gyrus fornicatus.

No results were made out on slipping the electrodes deeply between the hemispheres.

Corpus callosum.

Stimulation of this was likewise unattended by any outward result.

These complete the regions which I have as yet been able to succeed in experimenting on.

I now pass from experiments on the hemispheres to similar experiments on the basal ganglia. These were reached in a few cases by division of the corpus callosum, turning out the hemisphere partially, and thus laying open the interior of the lateral ventricle.

Corpus striatum.

The corpus striatum was laid bare and stimulated in VIII. and XIII.

Results :—

VIII. Left corpus striatum.

Stimulation caused bending of the body to the right (pleurosthotonus) and rigidity of the limbs in the position of flexion.

XIII. Right corpus striatum.

Curving of the head and trunk to the left, the platysma being strongly in action, while the limbs were maintained in a rigid condition in the position of flexion.

The results indicated that all the muscles were simultaneously in action, individually stimulated by irritation of the cortical centres.

Optic thalamus.

Stimulated in VIII. and XIII. Results :—

IX. Entirely negative.

XIII. Also negative after several explorations of the upper surface.

Application of the electrodes to the inner aspect in the region of the soft commissure caused a spasmodic extension of the limbs. There was no cry of pain. The result was not constant, and it may therefore have been an accidental complication.

No other experiments were made on these ganglia in the monkey, on account of their resemblance to the results obtained on other animals.

Corpora quadrigemina.

These ganglia were subjected to experimentation in the following seven cases, viz. V., VI., VIII., IX., X., XII., XIII., with the results :—

V. In this case the exploration was not sufficiently definite, as the exact position of the electrodes was not observed, and death occurred before a more careful exploration could be made.

The application of the electrodes to the ganglia on the left side (position as to the testes or nates not ascertained) caused the animal to utter various barking, howling, or screaming sounds of an incongruous character.

The head was drawn back and to the right, and the right angle of the mouth was strongly retracted while the stimulation was kept up. The tail was raised and the limbs were thrown into contortions, but nothing further was ascertained, as the animal died from hæmorrhage.

VI. In this case irritation of the right anterior tubercle (nates) caused intense dilatation of both pupils (especially beginning in the left), elevation of the eyebrows, and turning of the eyeballs upwards and to the left, at the same time that the head was turned in the same direction with an intensely pathetic expression.

Momentary application of the electrodes to the posterior tubercles (testes) caused the animal to bark loudly, the sound passing with longer stimulation into every conceivable variation of howling and screaming.

Continuous application of the electrodes for several seconds caused ultimately firm clenching of the jaws, retraction of the angles of the mouth (particularly the left), elevation of the eyebrows, and retraction of the ears. The pupils were dilated, eyes widely open, and the head thrown back. The tail became elevated, the limbs, after contortions of various kinds, became rigidly drawn back, the arms drawn back and flexed at the elbows and closely approximated to the sides. A complete state of opisthotonus was induced. The dilatation of the pupils occurred on irritation of both nates and testes; the screaming &c. only on irritation of the testes.

VIII. The results in this case were essentially the same as in VI., as regards the dilatation of the pupils, howling, and rigidity of the limbs, &c.

IX. As before, stimulation of the anterior tubercle on the right side caused elevation of the eyebrows, dilatation of the pupils, and turning up of the eyes to the left. Irritation of the ganglia for some time caused a condition of opisthotonus, and the phenomena described under VI.

Irritation of the testes caused utterance of every variety of barking and howling, ultimately trismus and general opisthotonus.

X. Exactly as in IX.

XII. As before, irritation of the testes caused barking and howling.

When the animal was nearly dead irritation of the testes caused only powerful retraction of the angles of the mouth, so as to show the firmly clenched teeth.

XIII. In this case the results as to the nates and testes were in every respect similar to those already detailed in the former cases.

Cerebellum.

Experiments were also made on the cerebellum in five monkeys, some of those already alluded to, as well as others. Further than stating that the results which I have already described (West-Riding Reports) in the case of rabbits, viz. alteration of the optic axis in different directions according to the part stimulated, are confirmed in the case of monkeys, I do not at present intend entering into fuller details, but reserve a full consideration of this subject for a future paper.

There is great difficulty in ascertaining the exact causation and relation of the phenomena which are manifested on irritation of the cerebellum along with movements of the eyeballs. Among others, it may be stated, are certain movements of the limbs and trunk, which I interpret as indications of an attempt to adjust the equilibrium of the body in harmony with the ocular movements. On this point, however, further experiments are necessary.

An attempt to analyze and interpret the significance of these results will be made in the next communication.

May 13, 1875.

Dr. J. BURDON SANDERSON, Vice-President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

In pursuance of the Statutes, the names of the Candidates recommended for election into the Society were read from the Chair as follows:—

William Archer, M.R.I.A.
James Risdon Bennett, M.D.
Dietrich Brandis, Ph.D., F.L.S.
James Caird, C.B.
Prof. John Casey, LL.D.
August Dupré, Ph.D., F.C.S.
James Geikie, F.R.S.E.
James Whitbread Lee Glaisher, M.A.
John Baboneau Nickterlien Hennessey, F.R.A.S.
Emanuel Klein, M.D.
E. Ray Lankester, M.A.
George Strong Nares, Capt. R.N.
Robert Stirling Newall, F.R.A.S.
William Chandler Roberts, F.C.S.
Major-General Henry Y. D. Scott, R.E., C.B.

THE CROONIAN LECTURE, "Experiments on the Brain of Monkeys" (Second Series), was delivered by DAVID FERRIER, M.A., M.D., Professor of Forensic Medicine, King's College. Communicated by Dr. SANDERSON, V.P.R.S. Received April 27, 1875. The following is an Abstract:—

This paper contains the details of experiments on the brain of monkeys, supplementary to those already laid before the Society by the author. They relate chiefly to the effects of destruction, by means of the cautery, of localized regions previously explored by electrical stimulation.

Twenty-five experiments are recorded in detail, and the individual experiments are illustrated by appropriate drawings. The results are briefly summed up as follows:—

1. Ablation of the frontal regions, which give no reaction to electrical stimulation, is without effect on the powers of sensation or voluntary motion, but causes marked impairment of intelligence and of the faculty of attentive observation.

2. Destruction of the grey matter of the convolutions bounding the fissure of Rolando causes paralysis of voluntary motion on the opposite side of the body; while lesions circumscribed to special areas in these convolutions, previously localized by the author, cause paralysis of voluntary motion, limited to the muscular actions excited by electrical stimulation of the same parts.

3. Destruction of the angular gyrus (*pli courbe*) causes blindness of the opposite eye, the other senses and voluntary motion remaining unaffected. This blindness is only of temporary duration, provided the angular gyrus of the other hemisphere remains intact. When both are destroyed, the loss of visual perception is total and permanent.

4. The effects of electrical stimulation, and the results of destruction of the superior temporo-sphenoidal convolutions, indicate that they are the centres of the sense of hearing. (The action is crossed.)

5. Destruction of the hippocampus major and hippocampal convolution abolishes the sense of touch on the opposite side of the body.

6. The sense of smell (for each nostril) has its centre in the subiculum cornu ammonis, or tip of the uncinate convolution on the same side.

7. The sense of taste is localized in a region in close proximity to the centre of smell, and is abolished by destructive lesion of the lower part of the temporo-sphenoidal lobe. (The action is crossed.)

8. Destruction of the optic thalamus causes complete anæsthesia of the opposite side of the body.

9. Ablation of the occipital lobes produces no effect on the special senses or on the powers of voluntary motion, but is followed by a state of depression and refusal of food, not to be accounted for by mere constitutional disturbance consequent on the operation. The function of these lobes is regarded as still obscure, but considered to be in some measure related to the systemic sensations. Their destruction does not abolish the sexual appetite.

10. After removal both of the frontal and occipital lobes, an animal still retains its faculties of special sense and the powers of voluntary motion.

The Society then adjourned over the Whitsuntide Recess, to Thursday, May 27th.

May 27, 1875.

JOSEPH DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

Mr. I. Lowthian Bell and the Right Hon. Sir James Colvile were admitted into the Society.

The following Papers were read :—

I. "On the Liquation of Alloys of Silver and Copper." In a Letter addressed to the Secretaries of the Royal Society, by Col. J. T. SMITH, Madras Engineers, F.R.S. Received April 2, 1875.

It has occurred to me that it might be useful, as a guide to future inquiries, if I were to communicate, in reference to Mr. Roberts's paper "On the Liquation, Fusibility, and Density of certain Alloys of Silver and Copper," the results of some experiments made by me many years ago, the conclusion to which I was led being that the separation of the constituent parts of an alloy containing $91\frac{2}{3}$ per cent. of silver was not so much due to the rapidity or slowness with which the heat of the fluid metal was abstracted, as to the inequality affecting its removal from the different parts of the melted mass in the act of consolidation. Thus, if a crucible full of the melted alloy were lifted out of the furnace and placed on the floor to cool, the surface of the melted metal within it being well covered with a thick layer of hot ashes, the lower parts of the mass, after it had become solid, would be found to contain less silver in proportion than the upper surface.

If, on the other hand, the crucible were left to cool while imbedded in the furnace, the upper surface only being exposed to the air, except a thin layer to protect it from oxidation, then the lower parts would, after solidification, be found finer than the upper surface.

The variations here referred to are not considerable; but they sometimes become of practical importance, especially in those cases wherein, as the practice of the Indian Mints used to be, the value of a large mass of coins is calculated by the assay of samples cut from a representative bar, formed by melting together a number of the pieces selected from the mass.

Under certain conditions, different parts of a bar of 50 or 60 lbs. weight, cast horizontally, though composed of metal which, previous to being poured from the crucible, was perfectly homogeneous, might be found to vary as much as $1\frac{1}{2}$ or even 2 per cent. A much smaller difference than this might be the cause of considerable loss or gain in the valuation of a large invoice.

This peculiar action in the cooling of melted silver alloy first attracted notice by observation of the fact that coinage-ingots, which were about 15 inches high, $2\frac{1}{2}$ inches broad, and $\frac{5}{8}$ inch thick, cast in vertical iron moulds, were *uniformly* finer at their upper surface and coarser at their sides and bottom, especially at the corners.

It was at first thought possible that this might be due to the combustion of the oil employed to lubricate the moulds causing a sensible refinement of the metal, as the flames were frequently tinged by copper; but the same increased fineness of the tops of the ingots was found to exist when they had been poured into new iron moulds which had never

been lubricated, and also when burnt clay-moulds were used—the only case in which the phenomenon did not occur being when clay-moulds heated to redness were used, and the melted metal, instead of being poured in at the top, was caused to rise from the bottom upwards.

It was also found that, by using artificial means to cool the ingots from their upper surface only, the usual refinement was prevented, and the metal might be caused to become as inferior in quality at the top as, in the ordinary course, it would have been at the bottom.

In short, after many experiments, it was satisfactorily established that, whatever form the metal might take, the act of cooling caused a partial separation of the copper towards the surfaces from which the heat was abstracted, those parts of a bar or ingot being the finest which congealed the last.

The experiments here referred to were part of a series constituting a minute analysis of all the ordinary processes of a mint, with the view of discovering the unavoidable causes of the loss of precious metal, if any, and measuring their amount. In the course of the series more than 3700 assays were made of standard silver under various conditions; and the result was a demonstration of the fact that, with the exception of the very inconsiderable proportion left in the sweepings, which might be reduced, in silver coinage, to less than the twenty thousandth part, no loss whatever ought to occur; and hence that, if the quantity left in the "sweep" be ascertained and allowed for, every particle of silver intrusted to the Mint might be accounted for. This was actually done in 1854–55 by a coinage of more than 24 tons of silver into 3,458,000 pieces, when every ounce was reproduced, even taking into account the "turn of the scale" in assays given in favour of the purchaser.

But this close agreement between the receipts and deliveries of a mint requires not only an exact valuation of the bullion delivered to it, but also an equally exact valuation of the coins produced by it; and a somewhat curious result followed, as a corollary to the phenomenon above described—namely, that as the ingots showed a uniform difference in their various parts, so the laminated straps, formed by reducing the same ingots to one twelfth of their original thickness, show similar varieties, and the coins cut from the straps also.

A minute examination of all the various parts of laminated straps proved that there must be, in all cases, a difference, and in some cases an important one, in the fineness of the several parts of every coin, depending upon the position it occupied in the laminated strap from which it was cut. On a careful examination of the latter there was found to be, as in the ingots, a *systematic* variation between the finenesses of the parts rolled out from the top and bottom, and also between the sides and centre. In like manner, as the centre of an ingot is finer than the sides, so the central line of the strap formed from it is finer than the edges, and this to a degree frequently exceeding the allowed "remedy"

of 1 dwt. in the pound, or the $\frac{1}{220}$ part; and as the coins were cut in a double row down each strap, from top to bottom, it followed that every coin at one end of a transverse diameter touched the edge of the strap, or the coarser metal, and at the other end of the same diameter touched the interior or finer metal.

Thus the different parts of coins composed of standard silver vary essentially in fineness at different points of their circumference. If we were to call those parts of the coins north and south which, before they were cut, lay in the direction of the length of the strap, and those at right angles thereto east and west, it would give a correct idea of this peculiarity to say that there is no essential difference between the north and south sides of the piece, but a considerable one between the east and west, frequently amounting to $\frac{3}{4}$ dwt. in the lb., and in coins cut from certain portions of each strap to more than 1 dwt., or $\frac{1}{220}$, sometimes even to 2 dwts. From this circumstance it is evident that assays taken in the manner formerly used for the pyx examination of coins in some of the Indian Mints, by flattening one edge of a coin and cutting off a part of it for trial, may often lead to its unjust condemnation; and when the whole work of a mint of many thousands of pounds value is liable to rejection in consequence of an unfavourable report upon individual coins, which more than once occurred in Madras, it is obvious that a more correct method of ascertaining the average fineness of the whole outturn is very desirable.

For this purpose it was suggested that the samples for assay should be taken from the centre of each coin, or by a ring representing the whole circumference. But the true average fineness of the whole of a large silver coinage is much more easily and better arrived at by taking out a large number of the coins indiscriminately, and having melted them together into a perfectly uniform and homogeneous compound, proceed according to the following method, which was latterly adopted in India.

While the representative coins are undergoing fusion, a portion is taken out in its fluid homogeneous condition and granulated by pouring into cold water. A number of the granules are then selected for assay, and after being carefully dried and weighed, are wholly (that is, each granule in its integral state) dissolved in acid. The silver contained in them is afterwards separated as a chloride, and estimated in the usual way, the fineness of the mass being calculated by a comparison of the weight of pure silver thus ascertained with the original weight of the granules.

When carefully prepared, as above described, there is a near agreement in the finenesses of the single granules, which rarely differ from the mean fineness of the metal so much as two thousandths when individually assayed, more than half of them being found within one thousandth, and the average of a number consequently very close to the truth.

II. "Note on Reversed Tracings." By C. HANDFIELD JONES, M.B. Cantab., F.R.S. Received March 22, 1875.

Soon after I began to use the sphygmograph I was advised by a friend to have the spring (which only gave a pressure up to 200 grammes) changed for one which was capable of giving 400. While working with this one day I obtained from the pulse of a healthy man, æt. 52, the tracing shown in fig. I., the pressure employed being 384 grammes. It is evidently quite an ordinary tracing of a normal pulse, except that it is reversed. If the glass is turned round and viewed from the non-smoked side the tracing is all *en règle*; but viewed from the side on which it was drawn it is topsy-turvy sideways, if such a phrase be permissible. I could not comprehend it, and showed it to a friend more versed in sphygmography than I was then; but he could only say that I must have made some strange mistake. Other good observers thought the tracings "very extraordinary," but could give no explanation of their *raison d'être*." Not seeing how I could have erred, I varnished my slide and put it by, in the hope that I might get to understand it some day. I noted, however, the fact that the pressure was very high, viz. 384 grammes. The pressure I usually employ for average pulses is 84 grammes to 140. Continuing to make observations, I noticed occasionally that the lever, when moving, behaved in an unusual manner; instead of jerking upwards, it jerked downwards, while the elevation was gradual; and this behaviour I found was connected with reversed tracings like my first. One of these observations was made on a young man to show the effect of exertion (see fig. II.). The tracing taken before exercise, a hard run, is pretty ample, and in other respects normal; it is marked *a*. The two next tracings, *b* and *c*, were taken immediately after the run, with the same pressure, viz. 140 grammes, as employed for *a*; they are both reversed, but the notch is hardly apparent in *c*. The pressure was now weakened to 84 grammes, and tracing *d* taken, which is described normally, but, like *b* and *c*, is far less ample than *a*. The reduction in size of the tracing in the three latter observations is undoubtedly due to cardiac exhaustion, and may be noticed in several of the instances given in my paper in the Proceedings of the Royal Society, 1873 (vol. xxi. pp. 374–383). But the reversal of the tracing in *b* and *c* seems as if it must be attributed mainly to the employment of a pressure which had become excessive in relation to the systolic force. As soon as this was lessened, the tracing assumed its proper form.

The evidence of fig. III. is to the same effect. It represents two tracings taken by Mr. Hawksley of his own pulse, while I was present, with a strong-springed instrument. The upper (*a*) was taken with a pressure of 300 grammes, the lower (*b*), which is reversed, with 750 grammes. The notch is quite as marked in the second as in the first. But it is certain that excessive pressure does not always produce a reversed tracing,

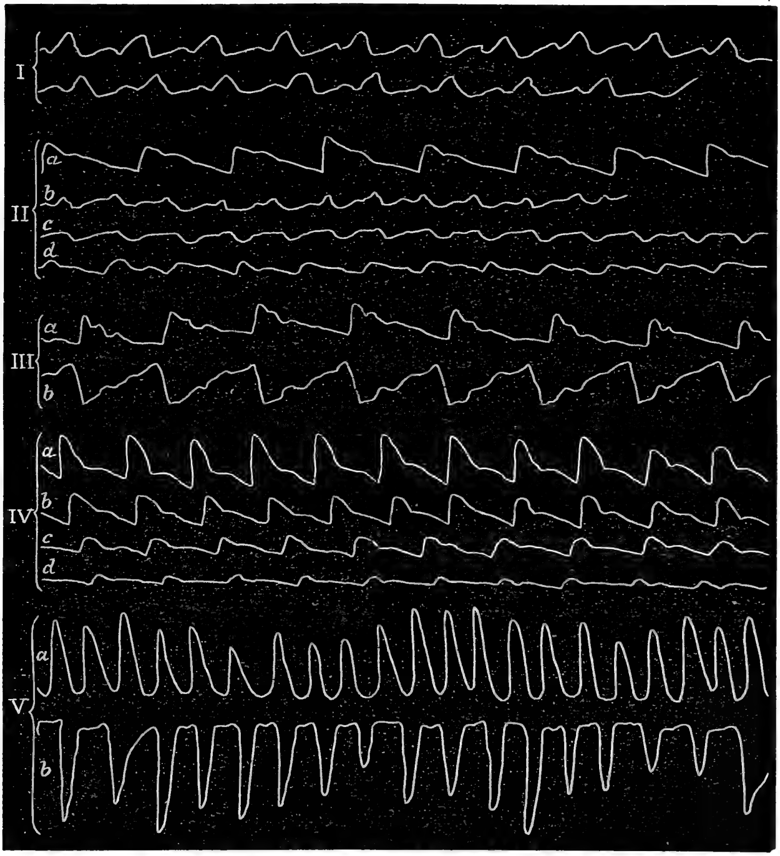


Fig. I. Reversed tracings taken from pulse of a healthy male, æt. 52. Pressur employed 384 grammes. 1871.

II. Tracings from pulse of a man, æt. about 25. Pressure employed for *a*, *b*, and *c* 140 grammes, for *d* 84 grammes. *a* is normal, *b* and *c* are reversed; *d*, taken two or three minutes later, is normal again.

III. Tracings of pulse of Mr. H., æt. about 35, healthy and robust. *a*, normal, with pressure 300 grammes; *b*, reversed, with pressure 750 grammes.

IV. Shows effect of increased pressure in lowering rise. Pulse of male, æt. 19, epileptic. *a* taken with pressure 56 grammes, *b* with 84 grammes, *c* with 112 grammes, *d* with 196 grammes.

V. Tracings taken with schema. *a*, spring resting on artery, pressure 2 oz., lever jerking upwards; *b*, plate resting on artery, pressure 30 oz., lever jerking downwards.

as is well exemplified in fig. IV.; it may even reduce the tracing to a straight line. Beyond this it seems as if the force of pressure "could no further go;" it is unintelligible how, after a sufficient force has been used to compress the artery, a greater amount of force should produce a fall and subsequent rise.

On further considering these tracings it seemed very important to determine whether the rise really corresponded to the rise of the normal tracing or not. The constant presence of the notch in the apparent rise in most cases suggested that this was perhaps not systolic in cardiac time, but diastolic; and on examination this proved to be the case; for while the sphygmograph applied to Mr. Hawksley's radial was giving well-marked falls of the lever, at a rate below 70 per minute, I observed, by feeling the other radial, that each *drop* of the lever coincided exactly with the beat of the pulse, and was therefore systolic in time, so that, of course, the rise must have been diasystolic. This conclusion was subsequently confirmed by observations made with a "schema," every contraction of the ball causing a fall of the lever under circumstances to be immediately mentioned. The "schema" employed was rather a rude affair, only a Higginson's syringe, with a long flexible tube attached, the open orifice of which was obstructed to make the pressure more effectual. When the elastic tube was placed under the *spring* of the sphygmograph every contraction of the ball jerked the lever up, and produced such a tracing as is represented in *a*, fig. V.; but when the tube was shifted a little forwards so as to lie, not under the spring, but under the brass plate just beyond it, then every contraction of the ball produced a sudden *fall* of the lever, as shown in *b*, fig. V. The pressure of the brass plate when *a* was obtained was little more than 2 oz., when *b* was obtained it was 30 oz. But that the increase of pressure was not the efficient cause of the lever falling instead of rising is shown by the fact that, in another observation, merely shifting the position of the elastic tube from below the spring to below the brass plate sufficed to make the difference. Simply raising the brass plate by taking hold of the screw attached to the framework above it also depressed the lever.

From observations recently made by Mr. Hawksley on his arm, we were unable for some time to obtain a reversed tracing, or, rather, the movement of the lever which gives it, although a very high pressure was employed, the spring being made to rest upon the radial artery. As long as this was the case the lever either remained motionless or jerked upwards; but when the brass plate at the distal extremity of the instrument was intentionally placed over the artery, the spring being set on one side, the lever was at once jerked downwards, and my friend, who had been sceptical before, was quite convinced that the cause of reversed tracings is the malposition of the instrument.

The only thing which makes me at all hesitate to rest in this conclusion is, that it does not explain why the reversal of the tracing sometimes

appears to be so closely connected with the existence of an undue amount of pressure. In fig. II. I find it rather difficult to think that the degree of pressure did not in some way modify the form of the tracing. Of course if, as is most probable, reversal of the tracing depends solely on the cause I have pointed out, and can be produced in no other way, no possible effect can be attributed to any alteration of the spring-pressure, as the spring is dissociated from the cause of motion.

The only notice I have found in Marey's work of any thing like such traces as I have described occurs at p. 282, where he says :—"Type 84 presents a singular peculiarity, the rebound has not had time to finish before the arrival of a fresh pulsation; the result is a form which might be taken for a tracing inverted (*écrit en sens inverse*); but this depends only on the phenomena of dicrotism not having had time to work themselves out (*s'accomplir*) between two successive pulsations."

The circumstance that the aortic notch is preserved during the arterial systole taking place under such altered circumstances is noteworthy, and shows the essentiality of this event. It was remarking the presence of this notch which first made me suspect that the sloping ascent of the inverted tracing was not equivalent to the ascent of the ordinary.

The chief practical lesson to be derived from the foregoing statements is, that we need, in using the Sanderson-Marey sphygmograph, to be very careful that the brass plate is so placed as not to rest upon the artery. If the artery is pressed on by the spring and the brass plate, the pressure of the latter being materially the greatest, the amplitude of the tracing may be factitiously increased. If the brass plate alone press on the artery the tracing will be reversed. I think the arch in the middle of the plate should be much wider than it is often made.

III. "Note on the Discharge of Ova, and its relation in point of Time to Menstruation." By John Williams, M.D. Lond., Assistant Obstetric Physician to University College Hospital. Communicated by Dr. Sharpey, F.R.S. Received April 7, 1875.

It is a recognized fact in physiology that ova are discharged in connexion with the menstrual function, but it is uncertain at what time in the course of the month the separation takes place. It is generally understood to occur towards the end of the discharge, or immediately after its cessation. I have, however, reason to believe, from observations made in several subjects, that such is not the case, but that it takes place before the appearance of the monthly flow with which it is connected. The cases which have come under my observation fall into four series, as follows:—

A. Cases, six in number, in which a Graafian follicle had been matured and actually ruptured.

(1) The first of these was a young girl who died through the effects of a fall, three or four days before the expected return of the catamenia. In the left ovary was a recently ruptured Graafian follicle. The cavity of the follicle was about $\frac{3}{4}$ inch in diameter, and contained a recent clot, which projected slightly through the rupture; the clot was of a fresh red colour, nowhere adherent to the parts around, for on making a section through the follicle it fell out. The wall of the vesicle was of a pale yellowish colour, and slightly wrinkled. The rupture had evidently taken place a short time only before death.

(2) The second subject was a woman who died suddenly through a fall, probably a fortnight after the cessation of the last menstrual flow. On examination a considerable quantity of blood was found in the cavity of the peritoneum, and the liver was torn. In the left ovary was a ruptured follicle, with corrugated and collapsed walls; its cavity contained no blood, but there was a slight effusion between its lining membrane and its outer coat. The depth of the follicle from the rupture to the furthest point of the opposite wall measured nearly $\frac{3}{4}$ inch. It is not impossible that this follicle was ruptured somewhat prematurely by the shock of the fall.

(3) The next example was observed during life. Mr. Christopher Heath performed ovariotomy on a patient on the fourteenth day after the cessation of the last catamenial discharge. Menstruation lasted usually three days, and the patient had always been regular every four weeks. In the ordinary course of things the next flow would have appeared in eleven days. When the diseased ovary had been removed, the remaining one was raised up, with a view to see if it were healthy, and it was observed that it contained an enlarged Graafian follicle, which became ruptured when being held in the hand. I ought to add that the flow returned three days after the operation, and eight days before it was due.

(4) The next case was a young woman who died of pleurisy on the fifth day of menstruation. On the surface of the left ovary was a rough, brownish-coloured, star-like cicatrix. On section there was seen under the cicatrix a corpus luteum, dilated in the middle and narrow at both ends, nearly $\frac{3}{4}$ inch in length and $\frac{1}{2}$ inch in width; its walls were in some parts of a pinkish and in others of a yellowish colour, slightly if at all thicker than those previously mentioned, and had small prominences on its inner surface. In the centre was a partially decolorized clot, which was but slightly adherent to the surrounding walls. From these characters it is evident that the rupture of the follicle had taken place several days before.

(5) The fifth member of this series was a patient who died on the fourth day of menstruation, and about the ninth of typhoid fever. One ovary contained a corpus luteum similar to the one just described.

(6) The last example occurred in a young girl, who died of pneumonia

six days after the cessation of the catamenia. On the surface of the right ovary was a small cicatrix, beneath which was a corpus luteum with the following characters:—It was of an irregular, elongated shape, nearly $\frac{1}{2}$ inch in length and $\frac{1}{4}$ in width; had thick, yellow, convoluted walls, and enclosed a small whitish mass, in which were two dark-coloured spots, which were evidently the remains of a clot. This ovary contained also a Graafian follicle of the size of a small pea. The determination of the age of effused blood is always difficult. In the Graafian follicle which becomes ruptured without impregnation taking place it is known that certain definite changes occur; the wall of the vesicle becomes thick, yellow, and convoluted; the blood which flowed into it and filled it becomes decolorized and absorbed. The exact length of time in which these changes in the follicle are brought about is not accurately determined, but it is known that the corpus luteum of one menstruation has become considerably atrophied by the return of the next.

It appears to me that the yellow body in the last example of this group was considerably older than the two preceding ones, and that it was more than a fortnight old, and that the two preceding ones were from eight to ten days.

B. Cases, four in number, in which a Graafian follicle had been matured, and hæmorrhage had taken place into its cavity, but no actual rupture had occurred.

(1) The first case was a patient who died of pyæmia in the third week after the cessation of the last catamenial flow. The left ovary contained a follicle $\frac{3}{4}$ inch in diameter, distended by a recent non-adherent, softish coagulum, uniform in consistence and colour. This follicle was prominent above the adjacent surface of the ovary; and its superficial wall was thick, and presented no tendency to point or rupture. There was no recent rupture to be seen on the surface of either ovary.

(2) The second example was a woman who had undergone an operation for fistula in ano. The monthly flow made its appearance a week before the expected time for its return, and she died five days after. One ovary contained a follicle measuring $\frac{5}{8}$ inch by $\frac{1}{3}$ inch; this follicle contained a bright red, fresh, loose clot, and its walls were thin and not corrugated. From these characters it appears that the hæmorrhage into the follicle had taken place but a short time before death.

(3) The next was a patient who had undergone an operation for the removal of an ovarian tumour. She died a fortnight after the operation, when she had menstruated for one day. At the inner extremity of the left ovary was a large, dark-coloured, softish mass, which, on section, proved to be a Graafian follicle containing a brick-red-coloured clot, which appeared to be of a spongy texture. It could with difficulty be turned out of the sac. After its removal it was seen that the wall of the sac was formed by a thin yellowish substance.

(4) The last example in this group was a person who suffered with fibroid tumour of the uterus. She died on the third or fourth day of the

menstrual flow. Both ovaries were bound to the surrounding structures by tough and firm false membranes. The left contained a follicle nearly an inch in length, in which was found a softish, dark-coloured clot, having a spongy texture, which appeared to be several days old.

In the first and third members of this group hæmorrhage had taken place into the follicle unquestionably before the appearance of the catamenial discharge.

In the second, hæmorrhage had occurred before the flow had become due; but the latter, owing to surgical interference, having returned a week before its time, the hæmorrhage took place while the discharge was in progress.

In the fourth, the condition of the clot makes it almost certain that the hæmorrhage had taken place before the appearance of the catamenia.

C. One case, in which a Graafian follicle had matured, but where neither rupture nor hæmorrhage had actually occurred.

This was a patient who died of typhoid fever just before the appearance of the catamenia. In one ovary there was an enlarged Graafian follicle, which was highly vascular, and projected like a nipple beyond the surrounding surface. It was evidently on the point of bursting, and it is doubtful whether rupture of the follicle or the appearance of the discharge would have taken place first.

D. Cases, three in number, in which no Graafian follicle had become enlarged to the size exhibited by it at maturity.

The first was a patient in whom the menstrual flow had almost ceased. There was no rupture in either ovary, but the right contained a Graafian follicle about the size of a small pea.

The next was a young suicide, who died three days after the cessation of the catamenial discharge. There was no recent rupture in either ovary, but the left contained a follicle similar to the one seen in the preceding case.

The last member of the series was a girl who died of peritonitis, caused by the rupture of an abscess on the right ovary. In the left was a Graafian follicle about the size of a small pea, but no recent rupture. The state of the lining membrane of the uterus showed that in this case menstruation was imminent.

Besides the appearances described, there were in all the preceding cases numerous Graafian follicles, varying in size from a millet-seed downwards, together with some superficial pits and atrophied corpora lutea.

These cases appear to me to bear out the opinion stated at the beginning of the paper, that, in the great majority of subjects, the discharge of ova takes place before the appearance of the menstrual flow with which it is connected; for in ten out of the fourteen rupture of a follicle, or hæmorrhage into its cavity, had occurred before the return of the catamenia; in one it was doubtful whether rupture of a follicle, or the appearance of the discharge would have taken place first; in two a menstrual period had passed without maturation of a follicle; and in one

a periodical discharge was imminent, though the ovaries contained no matured Graafian follicle. It is not improbable that the follicles which were found in the three last cases, and which were enlarged to the size of a small pea, would have become mature by the next return of the flow.

I have carefully considered the cases recorded by Cruikshank, Jones, Paterson, Lee, Girdwood, Negrier, Coste and others, and find that, though they do not contribute materially to the solution of the question discussed in this paper, yet, in so far as they go, they favour the view put forward here—a view which derives support from the custom imposed by the Levitical law, and observed to this day by the stricter sect of the Hebrew community.

Postscript. Received June 10, 1875. Communicated by Dr. Sharpey, F.R.S.

Since writing the above, I have had opportunities to examine two subjects in whom the date of the last menstruation was known.

The first was a girl aged 17 years, who died on the fifth day after admission to the Middlesex Hospital of traumatic tetanus. She was said to have ceased to menstruate just before admission; and the condition of the inner surface of the uterus confirmed that statement. The uterus and ovaries were small and imperfectly developed. On the surface of the right ovary was found a patch $\frac{1}{2}$ inch in diameter, slightly injected, and presenting a punctated appearance. In its centre was a cicatrix, appearing as a white spot, beneath which was situated a yellow body, elongated and irregularly flattened in shape. This appeared to be due to pressure from several Graafian follicles growing in close proximity to it, the largest of which was as large as a small pea. The yellow body measured nearly $\frac{1}{2}$ inch in length; it had folded walls, and in its centre was a thin elongated clot, the middle of which was of a dark colour.

The second subject was aged 26 years; she died of Bright's disease. The last menstruation began May 13th, ceased May 19th, and death occurred May 28th, 15 days after the appearance of the flow. Hæmorrhage had taken place into the superficial tissue of the ovaries, probably by reason of the condition of the blood.

In the right was a small superficial prominence formed by a yellow body, which measured about $\frac{3}{8}$ inch in diameter; it was throughout of a yellowish colour, and contained no trace of the colouring-matter of blood. On comparing these organs with one another and with those previously described, I am led to infer that in the first 12 to 14 days, and in the second about 20 days had elapsed since rupture of the follicle occurred.

Reichert has examined 23 organs in which signs of menstruation were recognizable. In four cases a Graafian follicle had matured but not ruptured, nor had hæmorrhage taken place, though the decidua menstrualis was in a state of greater or less development; in eighteen cases a Graafian follicle

had ruptured, and hæmorrhage had taken place into the decidua; in one case only, in which bleeding had not begun, had a Graafian follicle been ruptured. The latter statement appears opposed to the conclusions at which I have arrived; but this is only apparent; for in one case a follicle had ruptured, in four a Graafian follicle had matured before hæmorrhage began, and in one of these rupture was on the eve of taking place; in eighteen a follicle had ruptured, and hæmorrhage had taken place into the decidua menstrualis. Put in this form, Reichert's cases are not opposed to the conclusions arrived at in the preceding note; and as his cases have not been described, it is not possible to say what their actual bearing may be. The conclusion arrived at by Reichert, after examination of the 23 specimens, however, is that rupture of the Graafian follicle takes place at an early stage of the menstrual flow.

IV. "Note on Mr. Mallet's Paper on the Mechanism of Stromboli"*. By Robert Mallet, F.R.S. Received May 21, 1875.

Since the appearance of my paper on Stromboli some strictures hostile to the views therein contained have been published†, in which it is urged that the elevation of the fundus, or bottom of the visible crater, which I have assumed at 300 to 400 feet above the sea, is greatly below the truth, that being, it is affirmed, at least 2000 feet above the sea-level. It is added that "Mr. Mallet's whole theory hangs upon the proximity of the bottom of the crater to the sea-level," and that "Mr. Mallet's hypothesis of the mechanism of Stromboli is based entirely on these grossly inaccurate measurements." It is unnecessary that I should occupy the time of the Society by any discussion as to the correctness or incorrectness of either of the above levels, neither of which are more than loose approximations; but I beg permission to point out that the theory of the rhythmical action of Stromboli which I have proposed does not rest upon the proximity of the fundus to the sea-level, and stands equally valid whether the height of the tube C (see diagram no. 4, p. 512, Proc. Roy. Soc. 1874), or, what is the same thing, the difference in level between the sea and the bottom of the crater, be 300 to 400 feet or 2000 feet. That tube, whether long or short, is never filled, according to my views, by a column of liquid lava or water, but only with steam more or less dense before being blown off to a lower pressure; and the only change introduced by lengthening the tube C is, that a greater volume of steam is required to fill it; so that the tube being supposed of uniform calibre, the volume of steam required to produce equal tensions in the shorter or longer tube, as above stated, would be about as one to five; and no difficulty can suggest itself to the mind of any physicist as to the adequacy of the mechanism that I have suggested for such supply.

* Proc. Roy. Soc. 1874, vol. xxii. p. 496.

† Geol. Mag., Dec. 1874 and May 1875.

V. "Electrodynamic Qualities of Metals. (Continued from Phil. Trans. for February 28, 1856.)—Part VI. Effects of Stress on Magnetization." By Prof. Sir W. THOMSON, LL.D., F.R.S. Received May 27, 1875.

(Abstract.)

Weber's method, by aid of electromagnetic induction and a "ballistic galvanometer" to measure it, which has been practised with so much success by Thalén, Roland, and others, has been used in the investigation of which the results are at present communicated; but partial trials have been made by the direct magnetometric method (deflections of a needle), and this method is kept in view for testing slow changes of magnetization which the electromagnetic method fails to detect.

The metals experimented on have been steel pianoforte-wire, of the kind used for deep-sea soundings by the American Navy and British cable-ships; and soft-iron wires of about the same gauge, but of several different qualities.

I. *Steel.*

The steel wire weighs about $14\frac{1}{2}$ lbs. per nautical mile and bears 230 lbs. Weights of from 28 lbs. to 112 lbs. were hung on it and taken off, and results described shortly as follows were found:—

(1) The magnetization is diminished by hanging on weights, and increased by taking the weights off, when the magnetizing current is kept flowing.

(2) The residual magnetism remaining after the current is stopped is also diminished by hanging on the weights, and increased by taking them off.

(3) The absolute amount of the difference of magnetization produced by putting on and taking off weights is greater with the mere residual magnetism when the current is stopped than with the whole magnetism when the magnetizing current is kept flowing.

(4) The change of magnetization produced by making the magnetizing current always in one direction and stopping it is greater with the weights on than off.

(5) After the magnetizing current has been made in either direction and stopped, the effect of making it in the reverse direction is less with the weights on than off.

(6) The difference announced in (5) is a much greater difference than that in the opposite direction between the effects of stopping the current with weights on and weights off, announced in (4).

(7) When the current is suddenly reversed, the magnetic effect is less with the weights on than with the weights off.

II. *Soft-Iron Wires.*

Wires of about the same gauge as the steel were used, but, except one of them, bore only about 28 lbs. instead of 230. All of three or four kinds tried agreed with the steel in (1).

The first tried behaved (except a seeming anomaly, hitherto unexplained) in the reverse manner to steel in respect to (2), (4), (5), and (6); it agreed with the steel in respect to (7). Another iron wire*, which, though called "soft," was much less soft than the first, agreed with steel in respect to (1) and (2), but [differing from steel in respect to (3)] showed greater effects of weights on and off when the magnetizing current was flowing than when it was stopped.

Other soft-iron wires which were very soft, softer even than the first, agreed with all the steel and iron wires in respect to (1), but gave results when tested for (2) which proved an exceedingly transient character of the residual magnetism, and were otherwise seemingly anomalous.

The investigation is being continued with special arrangements to find the explanation of these apparent anomalies, and with the further object of ascertaining in absolute measure the amounts of all the proved effects at different temperatures up to 100° Cent.

The Society then adjourned over the Election-day, to Thursday, June 10.

June 3, 1875.

The Annual Meeting for the election of Fellows was held this day.

JOSEPH DALTON HOOKER, C.B., President, in the Chair.

The Statutes relating to the election of Fellows having been read, Mr. A. J. Ellis and Admiral Ommanney were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the lists.

The votes of the Fellows present having been collected, the following candidates were declared duly elected into the Society:—

* It was tested magnetically with weights up to 56 lbs., and broke, unfairly however, when 63 lbs. were hung on.

William Archer, M.R.I.A.
James Risdon Bennett, M.D.
Dietrich Brandis, Ph.D., F.L.S.
James Caird, C.B.
Prof. John Casey, LL.D.
August Dupré, Ph.D., F.C.S.
James Geikie, F.R.S.E.
James Whitbread Lee Glaisher, M.A.
John Baboneau Nickterlien Hennessey, F.R.A.S.
Emanuel Klein, M.D.
E. Ray Lankester, M.A.
George Strong Nares, Capt. R.N.
Robert Stirling Newall, F.R.A.S.
William Chandler Roberts, F.C.S.
Major-General Henry Y. D. Scott, R.E., C.B.

Thanks were given to the Scrutators.

June 10, 1875.

JOSEPH DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

Dr. James Risdon Bennett, Mr. James Caird, Mr. James Whitbread Lee Glaisher, Mr. J. Baboneau Nickterlien Hennessey, Mr. William Chandler Roberts, and Major-General Henry Young Darracote Scott were admitted into the Society.

The following Papers were read:—

I. "A Memoir on Prepotentials." By Prof. CAYLEY, F.R.S. Received April 8, 1875.

(Abstract.)

The present memoir relates to multiple integrals expressed in terms of the $(s+1)$ ultimately disappearing variables $(x \,.\,.\, z, w)$, and the same number of parameters $(a \,.\,.\, c, e)$, and being of the form

$$\int \frac{\rho \, d\varpi}{\{(a-x)^2 \,.\,.\, + (c-z)^2 + (e-w)^2\}^{\frac{1}{2}s+q}},$$

where ρ and $d\varpi$ depend only on the variables $(x \,.\,.\, z, w)$. Such an integral, in regard to the index $\frac{1}{2}s+q$, is said to be "prepotential," and in the particular case $q=-\frac{1}{2}$ to be "potential."

I use throughout the language of hyper-tridimensional geometry: $(x \,.\,.\, z, w)$ and $(a \,.\,.\, c, e)$ are regarded as coordinates of points in $(s+1)$-dimensional space, the former of them determining the position of an element $\rho d\varpi$ of attracting matter, the latter being the attracted point; viz. we have a mass of matter $=\int \rho d\varpi$ distributed in such manner that $d\varpi$ being the element of $(s+1)$- or lower-dimensional volume at the

point $(x \mathinner{..} z, w)$, the corresponding density is ρ, a given function of $(x \ldots z, w)$, and that the element of mass $\rho d\varpi$ exerts on the attracted point $(a \mathinner{..} c, e)$ a force proportional to the $(s+2q+1)$th power of the distance $\{(a-x)^2 \mathinner{..} +(c-z)^2+(e-w)^2\}^{\frac{1}{2}}$. The integration is extended so as to include the whole attracting mass $\int \rho d\varpi$; and the integral is then said to represent the prepotential of the mass in regard to the point $(a \mathinner{..} c, e)$. In the particular case $s=2$, $q=-\frac{1}{2}$, the force is as the inverse square of the distance, and the integral represents the potential in the ordinary sense of the word.

The element of volume $d\varpi$ is usually either the element of solid (spatial or $(s+1)$-dimensional) volume $dx \ldots dz\, dw$, or else the element of superficial (s-dimensional) volume dS. In particular, when the surface (s-dimensional locus) is the (s-dimensional) plane $w=0$, the superficial element dS is $=dx \ldots dz$. The cases of a less-than-s-dimensional volume are in the present memoir considered only incidentally. It is scarcely necessary to remark that the notion of density is dependent on the dimensionality of the element of volume $d\varpi$: in passing from a spatial distribution, $\rho\, dx \ldots dz\, dw$, to a superficial distribution, $\rho\, dS$, we alter the signification of ρ. In fact if, in order to connect the two, we imagine the spatial distribution as made over an indefinitely thin layer or stratum bounded by the surface, so that at any element dS of the surface the normal thickness is $d\nu$, where $d\nu$ is a function of the coordinates $(x \mathinner{..} z, w)$ of the element dS, the spatial element is $=d\nu\, dS$, and the element of mass $\rho\, dx \mathinner{..} dz\, dw$ is $=\rho\, d\nu\, dS$; and then changing the signification of ρ, so as to denote by it the product $\rho\, d\nu$, the expression for the element of mass becomes $\rho\, dS$, which is the formula in the case of the superficial distribution.

The space or surface over which the distribution extends may be spoken of as the material space or surface; so that the density ρ is not $=0$ for any finite portion of the material space or surface; and if the distribution be such that the density becomes $=0$ for any point or locus of the material space or surface, then such point or locus, considered as an infinitesimal portion of space or surface, may be excluded from and regarded as not belonging to the material space or surface. It is allowable, and frequently convenient, to regard ρ as a discontinuous function, having its proper value within the material space or surface, and having elsewhere the value $=0$; and this being so, the integration may be regarded as extending as far as we please beyond the material space or surface (but so always as to include the whole of the material space or surface)—for instance, in the case of a spatial distribution, over the whole $(s+1)$-dimensional space; and in the case of a superficial distribution, over the whole of the s-dimensional surface of which the material surface is a part.

In all cases of surface-integrals it is, unless the contrary is expressly

stated, assumed that the attracted point does not lie on the material surface; to make it do so is, in fact, a particular supposition. As to solid integrals, the cases where the attracted point is not, and is, in the material space may be regarded as cases of coordinate generality; or we may regard the latter one as the general case, deducing the former one from it by supposing the density at the attracted point to become $=0$.

The present memoir has chiefly reference to three principal cases, which I call A, C, D, and a special case, B, included both under A and C: viz. these are:—

A. The prepotential-plane case; q general, but the surface is here the plane $w=0$, so that the integral is

$$\int\frac{\rho\, dx\ldots dz}{\{(a-x)^2\ldots+(c-z)^2+e^2\}^{\frac{1}{2}s+q}}.$$

B. The potential-plane case; $q=-\frac{1}{2}$, and the surface the plane $w=0$, so that the integral is

$$\int\frac{\rho\, dx\ldots dz}{\{(a-x)^2\ldots+(c-z)^2+e^2\}^{\frac{1}{2}s-\frac{1}{2}}}.$$

C. The potential-surface case; $q=-\frac{1}{2}$, the surface arbitrary, so that the integral is

$$\int\frac{\rho\, dS}{\{(a-x)^2\ldots+(c-z)^2+(e-w)^2\}^{\frac{1}{2}s-\frac{1}{2}}}.$$

D. The potential-solid case; $q=-\frac{1}{2}$, and the integral is

$$\int\frac{\rho\, dx\ldots dz\, dw}{\{(a-x)^2\ldots+(c-z)^2+(e-w)^2\}^{\frac{1}{2}s-\frac{1}{2}}}.$$

It is, in fact, only the prepotential-plane case which is connected with the partial differential equation

$$\left(\frac{d^2}{da^2}\ldots+\frac{d^2}{dc^2}+\frac{d^2}{de^2}+\frac{2q+1}{e}\frac{d}{de}\right)V=0,$$

considered in Green's memoir 'On the Attractions of Ellipsoids' (1835), and called here the prepotential equation. For this equation is satisfied by the function

$$\frac{1}{\{a^2\ldots+c^2+e^2\}^{\frac{1}{2}s+q}},$$

and therefore also by

$$\frac{1}{\{(a-x)^2\ldots+(c-z)^2+e^2\}^{\frac{1}{2}s+q}},$$

and consequently by the integral

$$\int \frac{\rho\, dx \ldots dz}{\{(a-x)^2 \ldots + (c-z)^2 + e^2\}^{\frac{1}{2}s+q}}, \qquad \text{(A)}$$

that is by the prepotential-plane integral; but the equation is *not* satisfied by the value

$$\frac{1}{\{(a-x)^2 \ldots + (c-z)^2 + (e-w)^2\}^{\frac{1}{2}s+q}},$$

nor, therefore, by the prepotential-solid, or general superficial, integral.

But if $q=-\frac{1}{2}$, then, instead of the prepotential equation, we have

$$\left(\frac{d^2}{da^2} \ldots + \frac{d^2}{dc^2} + \frac{d^2}{de^2}\right)\mathrm{V} = 0\,;$$

and this is satisfied by

$$\frac{1}{\{a^2 \ldots + c^2 + e^2\}^{\frac{1}{2}s-\frac{1}{2}}},$$

and therefore also by

$$\frac{1}{\{(a-x)^2 \ldots + (c-z)^2 + (e-w)^2\}^{\frac{1}{2}s-\frac{1}{2}}}.$$

Hence it is satisfied by

$$\int \frac{\rho\, dx \ldots dz\, dw}{\{(a-x)^2 \ldots + (c-z)^2 + (e-w)^2\}^{\frac{1}{2}s-\frac{1}{2}}}, \qquad \text{(D)}$$

the potential-solid integral, *provided that the point* $(a \ldots c, e)$ *does not lie within the material space*: I would rather say that the integral does *not* satisfy the equation, but of this more hereafter; and it is satisfied by

$$\int \frac{\rho\, d\mathrm{S}}{\{(a-x)^2 \ldots + (c-z)^2 + (e-w)^2\}^{\frac{1}{2}s-\frac{1}{2}}}, \qquad \text{(C)}$$

the potential-surface integral. The potential-plane integral (B), as a particular case of (C), of course also satisfies the equation.

Each of the four cases gives rise to what may be called a distribution-theorem, viz. given V a function of $(a \ldots c, e)$ satisfying certain prescribed conditions, but otherwise arbitrary, then the form of the theorem is that there exists and can be found an expression for ρ, the density or distribution of matter over the space or surface to which the theorem relates, such that the corresponding integral V has its given value, viz. that in A and B there exists such a distribution over the plane $w=0$, in C such a distribution over a given surface, and in D such a distribution in space. The establishment, and exhibition in connexion with each other, of these

four distribution-theorems is the principal object of the present memoir; but the memoir contains other investigations which have presented themselves to me in treating the question. It is to be noticed that the theorem A belongs to Green, being in fact the fundamental theorem of his memoir of 1835, already referred to. Theorem C, in the particular case of tridimensional space, belongs also to him, being given in his 'Essay on the Application of Mathematical Analysis to the theories of Electricity and Magnetism' (Nottingham, 1828), being partially rediscovered by Gauss in the year 1840; and theorem D, in the same case of tridimensional space, to Lejeune-Dirichlet: see his memoir "Sur un moyen général de verifier l'expression du potential relatif à une masse quelconque homogène ou héterogène," Crelle, t. xxxii. pp. 80–84 (1846). I refer more particularly to these and other researches by Gauss, Jacobi, and others in the course of the present memoir.

"On the Fossil Mammals of Australia.—Part X. Family Macropodidæ: Mandibular Dentition and Parts of the Skeleton of *Palorchestes,* with additional evidences of *Sthenurus, Macropus Titan,* and *Procoptodon.*" By Professor Owen, C.B., F.R.S. Received May 10, 1875.

(Abstract.)

In this "Part" the author gives additional evidences of extinct genera and species of Kangaroos defined in the two preceding Parts (VIII. and IX.). To the *Palorchestes Azael* he adds characters of the mandible and mandibular teeth, and gives a restoration of the entire skull; the pelvis, femur, tibia, calcaneum, and principal bones of the hind foot of this gigantic species are described and figured.

Of *Macropus Titan* the author restores the entire skull and femur. Of *Sthenurus Atlas* he describes and figures the incisor teeth, the deciduous dentition, and the fore part of the skull of a young individual: of the larger species of this genus, *Sthenurus Brehus,* the entire skull and dentition are restored. The "Part" concludes with the restoration of certain bones of the hind foot in a Kangaroo slightly exceeding the largest *Macropus major* in size (indicated as a *Macropus affinis*), in the *Phascolagus altus,* in *Palorchestes Azael,* and in the three species of *Procoptodon* (*Pusio, Rapha,* and *Goliah*). The paper concludes with remarks on the transitional character of the latter forms, as bridging the gap between the saltigrade and gravigrade groups of phytiphagous Marsupialia.

The paper is illustrated by subjects for thirteen plates.

III. "On the Organization of the Fossil Plants of the Coal-measures.—Part VII. *Myelopteris, Psaronius,* and *Kaloxylon.*" By W. C. Williamson, Professor of Natural History in the Owens College, Manchester. Received June 3, 1875.

(Abstract.)

In his 'Dendrolithen' Cotta first figured some supposed stems under the name of *Medullosa*, to one of which he gave the name of *Medullosa elegans*. Corda subsequently figured a portion of the same plant, in his 'Flora der Vorwelt,' under the name of *Palmacites carbonigerus*, in the belief that it was the stem of an arborescent palm. M. Brongniart next gave to the plant the name of *Myeloxylon*, and at the same time expressed strong doubts respecting its monocotyledonous character. Goeppert gave this plant the generic name of *Stengelia*. In 1872 Mr. Binney expressed his belief that the plant was "the rachis of a fern, or of a plant allied to one." At the Meeting of the British Association at Bradford, in September 1873, the author described this plant, and pointed out his reasons for believing it to be not only a fern, but to belong to the interesting family of the Marattiaceæ; and in the subsequent January Professor Renault read a description of the plant to the Academy of Sciences at Paris, when, on independent evidence, he arrived at the same conclusion, viz. that it was one of the Marattiaceæ. Slightly modifying M. Brongniart's generic name, M. Renault designates the plant *Myelopteris*.

The author has obtained well-marked examples of this plant from the Lower Coal-measures near Oldham, from a rachis nearly an inch in diameter to the smallest leaf-bearing twigs and leaflet-petioles. It consists of a mass of parenchyma encased in a hypodermal layer of sclerenchymatous prosenchyma, arranged in anastomosing longitudinal bands, separated, when seen in tangential sections, by vertically elongated areolæ of parenchyma, which latter has probably spread out as a thin epidermal layer investing the entire rachis. These fibrous bands project inwards with sharp wedge-shaped angles; and in some examples portions of them become isolated as free fibrous bundles, running vertically through the peripheral portion of the inner parenchyma of the rachis. Numerous vascular bundles run vertically through this parenchyma. In transverse sections, not distorted by pressure, these bundles are arranged in some degree of regular order. This is especially the case with a circle composed of the peripheral series of bundles. Their component vessels are spiral in the case of some of the smaller ones, and barred, very rarely reticulate, in a few of the larger vessels. Scattered abundantly throughout the parenchyma are numerous narrow intercellular gum-canals. The majority of these are isolated; but in most of the specimens there runs side by side with the vessels, and enclosed within the special cellular sheath which imperfectly encloses each bundle,

a canal, of varied sizes and shapes, which appears to have been originally a gum-canal, subsequently enlarged irregularly by the shrinking of the neighbouring tissues. In the larger and more matured petioles these vascular bundles are very conspicuous, both in transverse and longitudinal sections; but in the small, young, and terminal subdivisions of the rachides the vessels are so small as to be almost undistinguishable from the surrounding parenchyma, while the gum-canals of such examples are, on the other hand, conspicuously large. Transverse sections of the most perfect examples of these young rachides exhibit, on their upper surface, a rounded central ridge, flanked on either side by a longitudinal groove, which arrangements are so conspicuous in the corresponding portions of the petioles of the Marattiaceæ and of other ferns. The ultimate leaflet-petioles were given off at right angles to the central rachis from which they sprang, corresponding in this respect with *one* of the types described by M. Renault. The author has not yet found in Lancashire any of the large specimens that have been met with on the continent at Autun and in the localities whence M. Cotta obtained his examples. He has found a few and yet smaller fragments among the sections from Burntisland. The recognition of the Marattiaceous character of these plants excludes the *Palmaceæ* for the present from all claim to share in the glories of the carboniferous vegetation.

The second plant described by the author consists of clusters of roots with a portion of the outermost parenchymatous layer of the stem of a tree fern, corresponding to those of the *Psaronites* long known to characterize the upper carboniferous deposits of Autun and other localities. The roots of the Oldham specimen, to which the author has assigned the name of *Psaronites Renaultii*, consist of a well-defined cylinder of sclerenchymatous prosenchyma, within which has been a mass of more delicate parenchyma, in the centre of which was the usual vascular bundle. But what characterizes the specimens, distinguishing them from the numerous species described by Corda, is the existence of vast numbers of cylindrical hairs, each composed of a linear row of elongated cylindrical cells: these have obviously been the absorbent root-hairs of the living plant, which may possibly have been some species of *Stemmatopteris*; but of this there is as yet no evidence.

The author then describes a small but very remarkable stem, to which he assigns the name of *Kaloxylon Hookeri*. This is a slender stem, rarely more than from one seventh to one tenth of an inch in diameter. In its young state it consists of a central vascular axis which has an hexagonal section, and which is composed of numerous vessels of various sizes, each of which exhibits the reticulate form of the scalariform or barred type, and which is so common amongst the plants of the Coal-measures. No true barred or spiral vessels have yet been seen in the *Kaloxylon*. In the young twigs this vascular axis is surrounded by a mass of large-celled cortical parenchyma, which, in turn, is encased by an epidermal struc-

ture, composed of a double row of what have evidently been colourless cells, and which are elongated vertically, but with square ends.

In the more matured stems, the central vascular axis of the young twigs becomes the centre whence radiate six exogenously developed wedges of vascular tissue, each of which enlarges as it proceeds outwards and terminates at its outer extremity in a slightly rounded contour. Each wedge consists of a series of radiating vascular laminæ, separated by numerous medullary rays, which latter consist of long and, for the most part, single vertical rows of mural cells. These six exogenous wedges are separated from each other by a large wedge of cellular cortical parenchyma, the cells of which are elongated radially and have a somewhat mural arrangement. As those between any two contiguous wedges proceed outwards, they separate more or less definitely into two series, which diverge right and left to sweep round the peripheral extremity of each nearest exogenous wedge, meeting and blending with a similar set coming from the opposite side of the wedge. In doing this they form a sort of loop, enclosing a semilunar mass of smaller cells interposed between the loop and the outer end of the exogenous wedge. The author demonstrates that this enclosed cellular tissue is essentially a cambial layer, out of which all the new vessels and peripheral extensions of the medullary rays were developed. Young vessels are seen at its inner surface in process of formation. External to these two specialized cortical tissues there is, in these matured stems, a mass of the primitive cortical parenchyma seen in the youngest shoots, enclosed, as before, in a double layer of epidermal cells.

The author has traced the development of branches from this axis. They are given off from single exogenous wedges in a very peculiar, but eminently exogenous manner, the details of which are given in the memoir. But, besides these, other clusters of vessels are given off which have no exogenous development or radiating arrangement. It is not yet clear what these secondary vascular bundles signify.

The author points out the general resemblance between this development of the detached exogenous wedges and that of the 4-partite woody axes of the Bignonias of Brazil, demonstrating at the same time their very marked differences.

Though no traces of leaves have yet been discovered in connexion with these stems, the author has very little doubt that they belong to some Lycopodiaceous plant. The nature of the vessels and the simplicity of their arrangement alike indicate cryptogamic features, at the same time that their mode of development indicates, with remarkable distinctness, that we have here another example of that exogenous mode of development of which the author has already described so many modifications amongst the fossil stems from the Coal-measures. The occurrence of this physiological process of exogenous growth in a stem which, when matured, was little more than one tenth of an inch in diameter, shows

that its occurrence is not merely a question of the size of the plant, as some have supposed, but that it has a deeper meaning, and corresponds more closely than has been supposed with the exogenous developments seen equally in large and small examples of living plants.

IV. "Experiments on Stratification in Electrical Discharges through Rarefied Gases." By WILLIAM SPOTTISWOODE, M.A., Treas. R.S. Received May 27, 1875.

In the stratified discharges through rarefied gases produced by an induction-coil working with an ordinary contact-breaker, the striæ are often unsteady in position and apparently irregular in their distribution. Observations made with a revolving mirror, which I hope to describe on another occasion, have led me to conclude that an irregular distribution of striæ does not properly appertain to stratification, but that its appearance is due to certain peculiarities in the current largely dependent upon instrumental causes.

The beautiful and steady effects obtained by Mr. Gassiot with his Leclanché battery, and also more recently by Mr. De La Rue with his chloride-of-silver battery, have abundantly shown the possibility of stratification free from the defects above mentioned; but it must be admitted that the means employed by those gentlemen are almost gigantic. The present experiments were undertaken with the view of ascertaining, first, how far it was possible to approach towards similar results with instruments already at my command; and secondly, whether these would afford other modes of attack, beside the battery, on the great problem of stratified discharges.

The induction-coil used was an "18-inch" by Apps, worked occasionally by 6 large chloride-of-silver cells, kindly lent to me by Mr. De La Rue, but more usually by 10 or by 20 Leclanché cells of the smallest size ordinarily made by the Silvertown Company. I have also, in connexion with the same coil, 120 of the latter cells, connected in twenties for quantity, and forming 6 cells of 20 times the surface of the former: these work the coil with the ordinary contact-breaker very well, giving 11-inch sparks whenever required. A "switch" affords the means of throwing any of the three batteries in circuit at pleasure.

Having reason to think that the defects in question were mainly due to irregularity in the ordinary contact-breaker, I constructed one with a steel rod as vibrator (figs. 1 & 2, p. 456), having a small independent electromagnet for maintaining its action. The natural vibrations of the rods which were tried varied from 320 to 768 per second; while under the action of the battery-current and electromagnet they varied from 700 to 2500, or thereabouts, per second. The amplitudes of the vibrations were exceedingly small, in fact not exceeding ·01 of an inch; and it is to this fact,

Fig. 1.

End elevation of Contact-breaker. Half size (linear).

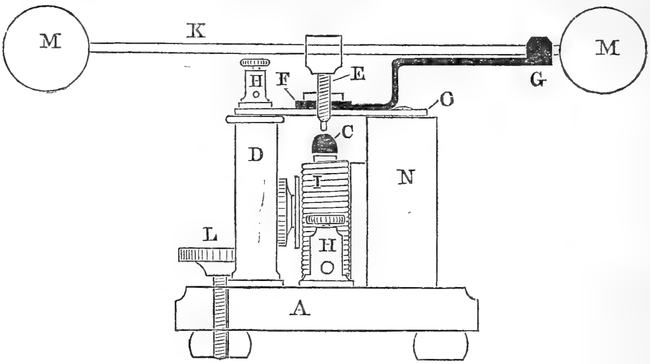
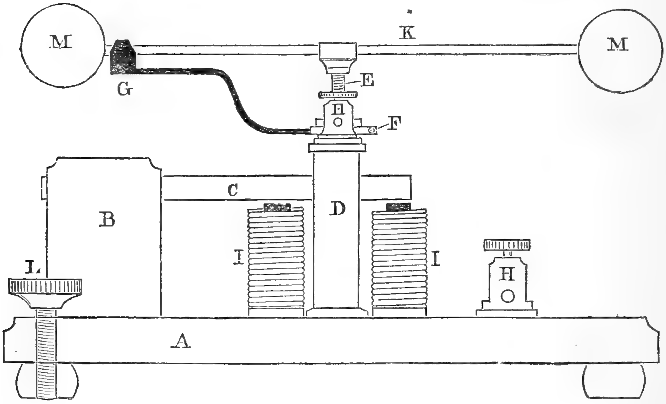


Fig. 2.

Side elevation of Contact-breaker. Half size (linear).



A. Mahogany base.
B. Heavy brass column for supporting vibrating spring.
C. Vibrating spring.
D. Brass column for supporting horizontal plate, O.
E. Platinum-tipped screw for contacts.
F. Friction-collar for holding steadying-arm.
G. Steadying-arm.
H H. Terminals.
I I. Electromagnet.
K. Lever-arm for fine adjustment.
L. Levelling-screw.
M M. Compensating weights.
N. Wooden pillar for electromagnet.

coupled with the extreme rapidity and consequent decision of make and break, that I mainly attribute the steadiness of the results.

The rod bore a plate of platinum, hammered hard, on its upperside; and when contact was made this plate met a thin platinum pin connected with the circuit. In order to avoid, as far as possible, any uncertainty in contact, the diameter of this pin was small; and one difficulty to be obviated was the heating and even fusion of the platinum when the circuit was completed. This was met by using the small-sized cells mentioned above, and employing a fine copper wire (No. 26) round the electromagnet. The very slight "strength of current," or minute "quantity," required for the illumination of vacuum-tubes made it possible so to reduce the surface of the battery-cells and the diameter of wire as to render the overheating in a great measure avoidable. This reduction of dimensions, however, is limited, first, by the amount of magnetism required to keep up the vibrations, and, secondly, by the diameter of the tubes used for the experiments; for it is obvious that, since wire and tube both form parts of the same circuit, in order to produce an equal illumination (other things being the same), we must for a tube of large diameter use a thicker wire than would be necessary for one of small diameter.

With a contact-breaker of this kind in good action, several phenomena were noticeable; but first and foremost was the fact that, in a large number of tubes (especially hydrocarbons), the striæ, instead of being sharp and flaky in form, irregular in distribution, and fluttering in position, were soft and rounded in outline, equidistant in their intervals, and steady in proportion to the regularity of the contact-breaker. These results are, I think, attributable more to the regularity than to the rapidity of the vibrations. And this view is supported by the fact that, although the contact-breaker may change its note (as occasionally happens), and in so doing may cause a temporary disturbance in the stratification, yet the new note may produce as steady a set of striæ as the first: and not only so, but frequently there is heard, simultaneously with a pure note from the vibrator, a strident sound, indicating that contacts of two separate periods are being made; and yet, when the strident sound is regular, the striæ are steady. On the other hand, to any sudden alteration in the action of the break (generally implied by an alteration in the sound) there always corresponds an alteration in the striæ.

It is difficult to describe the extreme delicacy in action of this kind of contact-breaker, or "high break," as it may be called. The turning through 2° or 3° of a screw, whose complete revolution raises or lowers the platinum pin through ·025 of an inch, is sufficient to produce or to annihilate the entire phenomenon. A similar turn in a screw forming one foot of the pedestal of the break is enough to adjust or regulate the striæ; and a slight pressure of the finger on the centre of the mahogany

stand, apparently rigid, or even on the table on which the contact-breaker stands, will often control their movements.

The discharges described above are usually (although not always) those produced by breaking contact; but it often happens, and that most frequently when the strident noise is heard, that the current produced by making contact is strong enough to cause a visible discharge. This happens with the ordinary as with the high break; but in the latter case the double current presents the very remarkable peculiarity that the striæ of one current are so arranged as to fit exactly into the intervals of the other; and, further, that any disturbance affecting the column of striæ due to one current affects similarly, with reference to absolute space, that due to the other, so that the double column moves, if at all, as a solid or elastic mass. And this fact is the more remarkable if we consider, as is easily observed in a revolving mirror, that these currents are alternate, not only in direction, but also in time, and that no one of them is produced until after the complete extinction of its predecessor. And it is also worthy of note that this association of striæ is not destroyed even when the two currents are separated more or less towards opposite sides of the tube by the presence of a magnetic pole. There seems, however, to be a tendency in that case for the striæ of one current to advance upon the positions occupied by those of the reverse current, giving the whole column a twisted appearance. But as there is no trace, so far as my observations go, of this association of alternate discharges when produced by the ordinary break, we seem led to the conclusion that a stratified discharge, on ceasing, leaves the gas so distributed as to favour, during a very short interval of time, a similar stratification on the occurrence of another discharge, whether in the same or in the opposite direction. An explanation of the fact that the striæ of alternate discharges occupy alternate and not similar positions is not obvious, and probably demands a better knowledge of the nature of the striæ than we possess at present.

The column of striæ which usually occupy a large part of the tube from the positive towards the negative terminal have hitherto been described as stationary, except as disturbed by irregularities of the break. The column is, however, frequently susceptible of a general motion or "flow," either from or towards the positive pole, say a forward or backward flow. A similar phenomenon was observed by Mr. Gassiot in some tubes with his large battery; but I am not acquainted with the exact circumstances under which it was produced. This flow may be controlled, both in velocity and in direction, by resistance introduced into the circuit, or by placing the tube in a magnetic field. The resistance may be introduced in either the primary or the secondary circuit. For the former arrangement I have successfully employed a set of resistance-coils supplemented by a rheostat. For the secondary current, as well as for the Holtz machine, I have used an instrument devised and constructed by my

assistant, Mr. P. Ward, to whose intelligence and skill I am much indebted throughout this investigation, intended for fine adjustment. Wherever the resistance be introduced the following law appears to be established by a great number and variety of experiments, viz. that, the striæ being previously stationary, an increase of resistance produces a forward flow, a decrease of resistance a backward flow. I have generally found that a variation of 3 or 4 ohms, or, under favourable conditions, of 1 or 2 ohms, in the primary current is sufficient to produce this effect. But as an alteration in the current not only affects the discharge directly, but also reacts upon the break, the effect is liable to be masked by these indirect causes. The latter, so far as they are dependent upon a sudden alteration of the resistance, may be diminished by the use of the rheostat; but when the striæ are sufficiently sensitive to admit the use of this delicate adjustment, some precautions are necessary to insure perfect uniformity of current, so as to avoid disturbances due to uneven contact in the rheostat itself.

When the striæ are flowing they preserve their mutual distances, and do not undergo increase or decrease in their numbers. Usually one or two remain permanently attached to the positive electrode; and as the moving column advances or recedes, the foremost stria diminishes in brilliancy until, after travelling over a distance less than the intervals between the two striæ, it is lost in darkness. The reverse takes place at the rear of the column. As the last stria leaves its position, a new one, at first faint and shadowy, makes its appearance behind, at a distance equal to the common interval of all the others: this new one increases in brilliancy until, when it has reached the position originally occupied by the last stria when the column was at rest, it becomes as bright as the others. The flow may vary very much in velocity; it may be so slow that the appearances and disappearances of the terminal striæ may be watched in all their phases, or it may be so rapid that the separate striæ are no longer distinguishable, and the tube appears as if illuminated with a continuous discharge. In most cases the true character of the discharge and the direction of the flow may be readily distinguished by the aid of a revolving mirror. In some tubes, especially in those whose length is great compared with their diameter, the whole column does not present the same phase of flow; one portion may be at rest while another is flowing, or even two conterminous portions may flow in opposite directions. This is seen also in very wide tubes, in which the striæ appear generally more mobile than in narrow ones. But in all cases these nodes or junction-points of the flow retain their positions under similar conditions of pressure and current; and it therefore seems that, under similar conditions, the column in a given tube always breaks up into similar flow-segments.

These nodes will often disappear under the action of a magnetic pole. Thus if the first segment, measured from the positive terminal, be sta-

tionary and the second be flowing backwards (*i. e.* from $-$ to $+$), a magnetic pole of suitable strength, placed at the distant end of the latter, will stop its flow, and the whole column will become stationary throughout. An increase in the strength of the magnet, or a nearer approach of it to the tube, will produce a general forward flow of the column.

The phenomena of the flow, as well as others of not less interest, are capable of being produced with the Holtz machine. It is well known that stratified discharges, similar to those produced by an induction-coil working with an ordinary break, may be produced by such a machine, provided that it be furnished with the usual Leyden jars, and a high resistance (usually a piece of wetted string) be interposed in the circuit. The absence of either of these conditions was supposed to destroy the striæ and to render the discharge continuous. Experiments which I have recently made, but do not describe on the present occasion, tend in part, but only in part, to confirm this view. They show that for the production of striæ both quantity and resistance are necessary, that the discharge must occupy a certain short, perhaps, but finite time, or, as it may also be expressed, that a continuous current is an essential element.

Now, seeing that every tube must offer some resistance, and also that by adjusting the height of the vertical condensors of the machine (or length of air-spark interposed in the circuit) we had the means of altering the quantity in the discharge, it seemed worth while to try whether, by a suitable adjustment of the parts, phenomena similar to those brought out by the coil and high break might not be produced by the machine. And this proved to be very easy of attainment in tubes which had been successfully used by the coil; and not only so, but the character of the flow therein shown confirmed in a very striking and simple manner the effects of resistance described above.

The connexions being made in the usual way, and no air-spark being admitted into the circuit, a vacuum-tube of carbonic oxide, about 60 centims. in length and 4·5 centims. in outside diameter, gave, when the plates of the machine revolved at about six times per second, a rather confused discharge. As the speed was increased, a rapid forward flow of the striæ was readily discerned; and on a still further increase to about ten revolutions per second, the flow, first in one part and then throughout nearly the whole length of the tube, slackened its pace and stopped, and ultimately reversed its motion. An increase of speed is equivalent to an overcoming or a diminution of resistance in the circuit, a diminution of speed to an augmentation of resistance. Hence the phenomena of flow produced by the machine agree with those produced by the coil.

It is unnecessary to detail the effects obtained with many other tubes, as they all agreed in their main features.

In a sulphurous-acid tube, which with the induction-coil and ordinary break gave broad flocculent striæ but unsteady and fluttering in posi-

tion, the effects with the Holtz machine were very striking: the striæ, with steady revolution of the machine, became fixed in position and well defined. This tube, some carbonic-acid-gas tubes, and one or two others, generally containing acid residua, form a class in which the action of the machine more nearly approaches that of Mr. Gassiot's battery than in any others. The striæ thus formed were not easily brought into a state of flow; but an increase in the rapidity of the machine, or a diminution of resistance, increased the number of the striæ. As the rapidity was augmented, the striæ might be seen pouring themselves out, as it were, from the positive pole; the length of the column was slightly increased, but by no means in proportion to the number of striæ.

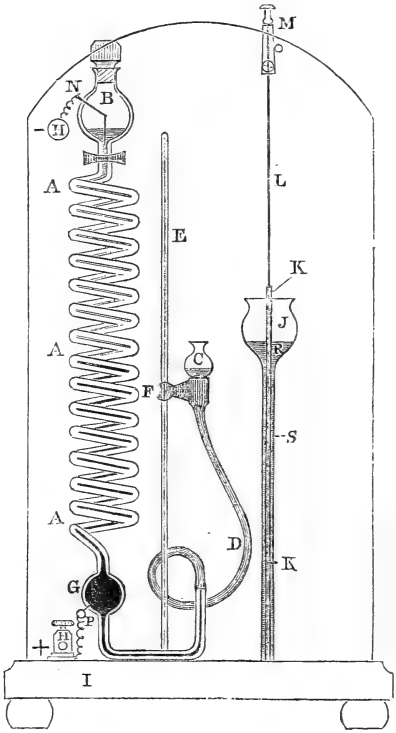
One modification of this effect, although almost fantastic in its appearance, seems to deserve a special notice. It is well known that if a sufficient interval of air be thrown into the circuit all trace of stratification disappears, and at the same time the dark interval between the positive and negative parts and the negative halo itself are obliterated. If, however, the interval of air be very small, the two kinds of discharge may be seen coexisting; a narrow column of the continuous discharge extends along the tube, and on it the striæ appear to be strung. These effects are easily produced by slightly lowering one of the vertical conductors of the machine; and perhaps the best effects are shown if the conductor on the side connected with the positive terminal of the tube is lowered. When this is done the striæ occupying the portion nearest to that terminal become widely separated at unequal and varying intervals; they appear to oscillate along the tube with independent motions, as if attached to an elastic string which at each instant is unequally stretched at its various parts. The portion of the column so affected varies with the length of the interval of air; and when, for instance, that portion amounts to two thirds of the entire length, the striæ in the remaining third appear crowded together. As the interval of air is further increased more striæ become disturbed, the continuous discharge becomes wider and more prominent, and ultimately overpowers and obliterates the striæ.

The resistance-coil used for the secondary current or the machine consists of a hollow glass spiral, A A A (fig. 3), having a length of about 50 inches and an internal diameter of $\frac{1}{40}$ of an inch. At the head is a small glass bulb B, having an opening at the top, which is closed by a glass stopper. A platinum wire, N, connected with one of the terminals, H, dips to the bottom of the bulb B, which is partially filled with sulphuric acid. C is a small glass bulb containing mercury, and is connected with the lower end of the spiral by a flexible tube, D. The height of this bulb can be regulated by means of the slide F, which moves on the steel rod E. P is a small platinum wire fused into the interior of the bulb G, and is in connexion with the other terminal, H.

If the bulb C be placed in any position, the mercury will rise in the spiral to the same level as that in the bulb. The mercury will act

as a metallic conductor, and a current flowing between the terminals will undergo the resistance due to the acid in the upper part of the spiral.

Fig. 3.

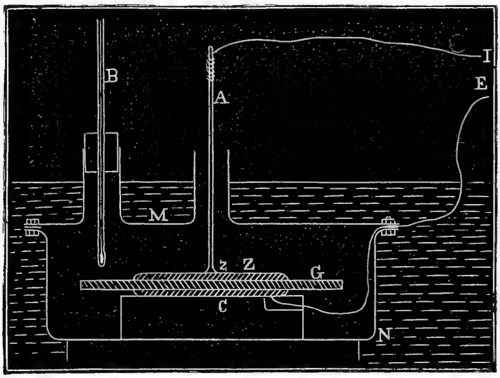


Attached to the same stand is a coarse resistance-tube, adapted also to currents of high tension, such as a 3- or 4-inch spark from an induction-coil. It consists of a tube, S, of about 4 millims. diameter, terminating at its upper end in a funnel-shaped bulb, J, and having a platinum wire fused into its lower end. This tube is filled to R with a mixture of glycerine and water, in the proportions of 6 to 1. L is a long steel rod, extending from the clip M to the bottom of the tube, and capable of sliding up and down; its lower half is coated with a very small glass tube, K K, so that its extremity alone is exposed to the contact of the fluid. If the steel rod L be in the position shown in the diagram, no resistance will be offered to a current passing through the system; but if it be raised, the resistance offered will be in proportion to the length of column of resisting fluid through which it has to pass.

V. "Electrolytic Conduction in Solids.—First Example. Hot Glass." By Prof. Sir WILLIAM THOMSON, F.R.S. Received June 10, 1875.

Many years ago I projected an experiment to test the voltaic relations between different metals with glass substituted for the electrolytic liquid of an ordinary simple voltaic cell, and with so high a temperature that the glass would have conducting-power sufficient to allow induction through it to rule the difference of potentials between the two metals. Imperfect instrumental arrangements, and want of knowledge of the temperature at which glass would have sufficient conductivity to give satisfactory results, have hitherto prevented me from carrying out the proposed investigation. The quadrant electrometer has supplied the first of these deficiencies, and Mr. Perry's recent experiments* on the conductivity of glass at different temperatures the second. The investigation has now been resumed; and in a preliminary experiment I have already obtained a very decided result.

The drawing shows the arrangement adopted. M N is a brass case immersed in an oil-bath. A copper plate, C, of 5 centims. diameter, lies in the case on a block of wood; it is kept metallically connected with the outside case, E, of the electrometer. A flint-glass plate, G, which is



found to insulate very well at ordinary temperatures, is laid upon C. A zinc-plate, Z, lies on the glass, and is connected with the insulated electrode, I, of the electrometer, by means of a wire attached to the end of a stout metallic stem, A Z, passing through the centre of an open vertical tube reaching above the level of the oil. The glass was heated gradually, and was usually kept between 100° and 120° C., the temperature being measured by a thermometer, B.

Even below 50° C. there is a decided result, but shown less rapidly

* See *infrà*, p. 468.

than at higher temperatures. If the glass is kept at 50° C. for some time, and I, after having been metallically connected with P, is left insulated, it soon becomes sensibly charged; and the charge increases till it is approximately equal to that acquired when zinc and copper plates in a liquid electrolyte are metallically connected with I and E respectively. With the hot glass, as with the liquid electrolyte, the charge given by the zinc to the insulated electrode of the electrometer is negative. The charge ultimately reached when the temperature is 50° is not exceeded at higher temperatures; but, as said above, when the zinc is connected with the copper and then insulated, the charge increases towards its ultimate value much more rapidly at higher temperatures than at lower.

At temperatures between 100° and 120° C. there is a sensible diminution of the ultimate charge after the zinc has been kept for a short time connected with the copper and then insulated. There is also a slow diminution of the ultimate, or, as we may now call it, the temporarily static, charge when the zinc plate is left insulated for several hours in connexion with I.

If a small quantity of either negative or positive electricity be given to I (always in metallic connexion with the zinc), the temporarily static state is reached at about the same rate as the zero would be reached by conduction through the hot glass (according to Mr. Perry's experiments, communicated to the present Meeting) were the plates both of copper or both of zinc.

After the experiment the surfaces of the copper and zinc plates in contact with the glass were found to be thickly oxidized. The glass plate was quite cloudy after the experiment, and a repetition of the experiment increased its cloudiness. This plate is the flattened bottom of a flint-glass electrometer-jar.

Three smoother glass plates, tried since, show as yet no signs of decomposition. At first they only became "exhausted" (in their power to produce the normal charge in zinc and copper) after the plates had been connected for nearly a day, the glass being at from 100° to 120° C.; but after a time, although they still gave the normal charge at the beginning of the morning's experiments, the charge fell to zero quite rapidly (that is, in about an hour), even when the zinc was kept insulated.

Keeping for a length of time the zinc charged negatively (so as to give to I a greater negative charge than that which it would have in the "temporarily static" condition of the copper, hot glass, and zinc) seemed to have no effect in restoring the normal electrolytic condition; but I propose to pursue this trial further, especially with longer time for the restorative electrification.

I propose also to return to similar experiments which I made many years ago on the electric relations of copper, ice, and zinc.

VI. "Note on Dulong and Petit's Law of Cooling." By DONALD MACFARLANE. Communicated by Prof. Sir W. THOMSON, F.R.S. Received June 10, 1875.

The 'Journal de Physique' for December 1873 contains a friendly notice by Professor A. Cornu of experiments made to determine surface-conductivity for heat (or, as we may call it, "thermal emissivity") in absolute measure, an account of which was communicated to the Royal Society, and read January 1872 (see Proceedings, vol. xx. p. 90). On the results there given M. Cornu remarks:—

"Ces nombres vérifient la conclusion de Dulong et Petit, à savoir que les vitesses de refroidissement ne dépendent de l'état des surfaces que par une constante de proportionalité.

"L'accélération négative du rapport des pouvoirs emissifs n'infirme pas sensiblement cette conclusion; elle est si faible qu'elle peut être attribuée à une petite erreur régulière dans l'évaluation des différences de températature; en effet, l'auteur ne paraît tenir aucun compte d'une cause délicate d'erreur qui avait préoccupé Dulong et Petit, à savoir la résistance inégale à la transmission de la chaleur dans les deux cas. Il est évident que, dans le refroidissement le plus rapide, la température est distribuée moins uniformément que dans le cas d'un refroidissement lent; l'aiguille thermoélectrique indique donc moins bien la température moyenne de la masse que les boules de mercure des physiciens français."

On this it is to be remarked that a rigorous proportionality in the rates of cooling of different surfaces is in itself not probable; and my experiments in fact disprove it, so far as it is not at all likely that the errors of observation could be so great or so consistently regular in the same direction as the truth of the supposed law would require.

As to the variation of temperature from centre to surface occasioned by the rapid cooling of the ball, this was certainly not overlooked in planning the experiments. Sir William Thomson considered the matter carefully, and selected copper, on account of its high conductivity, estimating that in a copper ball of the dimensions used (diameter 4 centimetres) the temperature must be sensibly uniform throughout. A very simple calculation (made in consequence of M. Cornu's criticism, and appended below) from Fourier's celebrated formula for the cooling of a homogeneous solid globe shows, in fact, that, in the case of a copper globe of 2 centimetres radius, the centre is warmer than the surface by only about $\frac{1}{4000}$ of the excess of its temperature above that of the surrounding medium. There would be a much greater difference of temperature between surface and centre in a globe of mercury of the same dimensions, because mercury is a much worse conductor of heat than copper, and because a much greater difference of temperatures than that which there is in the copper would be required to produce any considerable convection of heat by currents in the liquid. Moreover the glass envelope con-

taining the mercury in a thermometer-bulb of ordinary dimensions produces a sensible difference of temperature between the outer surface of the glass exposed to the external medium and the surface of the mercury. For let b be the thickness of the glass, E the "emissivity" of its outer surface, and k the conductivity of its substance; let the excess of temperature of the outer surface of the glass above that of the surrounding medium be v, and the excess of temperature of the inner surface of the glass above the outer δv; we have

$$k\frac{\delta v}{b}=\mathrm{E}v.$$

Now by the Glasgow experiments it has been found that E is approximately $\frac{1}{4000}$ of a gramme-water thermal unit per square centimetre per second; and by the determinations of conductivities of stones and underground strata in absolute measure by Peclet and Forbes the value of k for glass may be roughly estimated at $\frac{1}{400}$, in terms of centimetre, second, and gramme-water thermal unit. Hence

$$\frac{\delta v}{v}=\frac{1}{10}b.$$

Thus, if the thickness of the glass be half a millimetre (i. e. $b=\frac{1}{20}$), we have

$$\delta v=\frac{1}{200}v.$$

This is a small difference, but by no means imperceptible in the delicate experiments of Dulong and Petit; and it is twenty times the difference of temperature between the centre and surface of the cooling copper globe of 4 centimetres diameter.

Appendix.

Distribution of temperature in a cooling copper globe of 4 centimetres diameter, calculated from Fourier's formula

$$v=\Sigma \mathrm{P}_i\frac{\sin\theta_i}{\theta_i}\epsilon^{-\rho_i t}, \quad \ldots \ldots \ldots \quad (1)$$

where

$$\rho_i=\frac{\varpi_i^{\,2}k}{a^2c}, \quad \theta_i=\frac{x}{a}\varpi_i,$$

ϖ_i roots of the transcendental equation

$$\frac{\varpi}{\tan\varpi}=1-\frac{\mathrm{E}a}{k}, \quad \ldots \ldots \ldots \quad (2)$$

and P_i coefficients determined to give (according to Fourier's method)

any arbitrary function of x from $x=0$ to $x=a$, for the value of v, when $t=0$:

> v temperature at time t and distance x from centre of globe,
> a the radius of the globe,
> k the thermal conductivity of its substance,
> c the thermal capacity per unit volume of its substance,
> E the thermal emissivity of its surface.

Taking centimetre, second, and gramme-water thermal units for the fundamental units, we have, as stated above,

$$\mathrm{E}=\frac{1}{4000} \text{ (rough approximation)};$$

and Ångström's experiments gave for copper

$$k=1 \text{ approximately.}$$

Therefore

$$\frac{\mathrm{E}a}{k}=\frac{a}{4000};$$

and for the globe of 4 centimetres diameter used in the Glasgow experiments,

$$\frac{\mathrm{E}a}{k}=\frac{1}{2000}.$$

In all cases in which $\frac{\mathrm{E}a}{k}$ is small, the smallest root of the transcendental equation (2) is approximately equal to

$$\sqrt{\frac{3\mathrm{E}a}{k}}.$$

Calling this ϖ_1, we have

$$\rho_1=\frac{3\mathrm{E}}{ac}$$

and

$$\frac{\sin\theta_1}{\theta_1}=1-\frac{1}{6}\theta_1^{\,2} \text{ approximately,}$$

$$=1-\frac{1}{2}\frac{\mathrm{E}a}{k}\frac{x^2}{a^2}.$$

Now any chosen term of (1) is a particular solution of the problem; that is to say, it is *the* solution for the case for which the initial distribution of temperature is that which it expresses when $t=0$. Hence

$$v=\left(1-\frac{1}{2}\frac{\mathrm{E}a}{k}\frac{x^2}{a^2}\right)\epsilon^{-\frac{3\mathrm{E}}{ac}t}$$

expresses the temperature at time t, if when $t=0$ the temperature is expressed by

$$v=1-\frac{1}{2}\frac{\mathrm{E}a}{k}\frac{x^2}{a^2}.$$

Taking, for instance, the copper globe of 4 centimetres diameter, we have

$$v=\left(1-\frac{1}{4000}\frac{x^2}{a^2}\right)\epsilon^{-\frac{3\mathrm{E}}{ac}t}; \quad \ldots\ldots\ldots \quad (3)$$

and we see that in the Glasgow experiments the difference of temperatures between surface and centre was just $\frac{1}{4000}$ of the excess of either above the temperature of the surrounding medium, when time enough had elapsed to allow the first term of Fourier's series to be the predominating one. *Before* that time the difference of temperatures must have been *less* than $\frac{1}{4000}$ of either, if initially the temperature was uniform from surface to centre. The Fourier analysis of the transition from the supposed initial uniform distribution to the state represented by (3) is exceedingly interesting, but unnecessary for the settlement of the present question.

VII. "Preliminary Results of an Investigation on the Electric Conductivity of Glass at different Temperatures." By John Perry, B.E., Thomson Experimental Scholar in The Natural Philosophy Laboratory at Glasgow*. Communicated by Professor Sir William Thomson, F.R.S., LL.D. Received April 8, 1875.

A quadrant electrometer now in use in the laboratory seems to retain its whole charge from day to day; a week's loss is just perceptible, and may be supplied by a few turns of the replenisher. In a guard-ring electrometer now in use the charge is almost wholly retained from week to week. These qualities are due to the exceptionally great insulation-resistance of the glass employed.

At various times experiments have been made in the laboratory at Glasgow to determine the insulation-resistance of different kinds of glass. Of the specimens hitherto examined, those of flint glass have insulated best; and it is hoped that experiments on flint glass now being proceeded with will define the most suitable glass for use in electrometers and other electrical instruments.

The method of investigation is essentially the same as that which was adopted by Sir William Thomson some years ago. B D A (fig. 1) is a

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hollow glass globe with a long stem. C is a brass insulator with pumice and sulphuric acid, to keep the stem free from moisture. C is supported on a stand resting on the table. A wooden clamp supports A B at A. B D is covered with tinfoil or with wet linen cloth. W is water inside the globe.

Fig. 1.

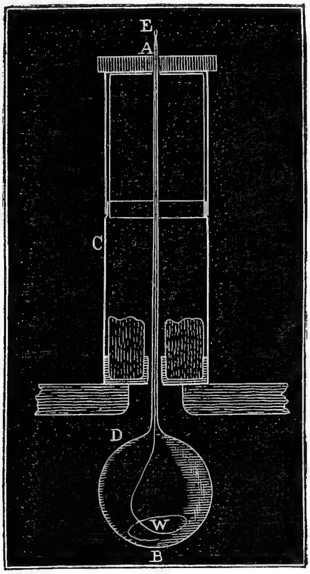
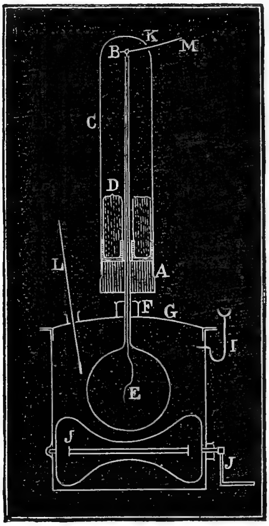


Fig. 2.



For a lecture illustration Mr. M'Farlane, on March 13th, 1874, charged a flint-glass jar, the globe of which was 13 centims. in diameter and about 0·13 centim. thick. The inside coating was put to earth by means of a wire; the outside was connected with the electrometer and then charged. The wire was then withdrawn from the inside, and the stem was sealed at E. On March 20th B D was again insulated and connected with the electrometer; the stem was broken at E, and the inside put to earth as before. The original charge was 2170. At the end of a week the charge was found to be 1952. The week's loss was 218, or 10 per cent.

The jar was again sealed on March 20th with a charge of 1875. On April 7th the charge was 1332; so that the loss in 18 days was 543, or 28 per cent. of the whole charge.

On Jan. 5th, 1875, the author gave a charge of 1465 to a flint-glass jar. On March 16th the electricity had all disappeared. Another flint-glass jar charged to 1048 on Jan. 5th, when opened on March 16th had a charge of 144, the loss in 70 days being 904, or 86 per cent.

Twenty flint-glass jars, of the shape shown in fig. 1, are now being examined. The composition of the glass of each jar is known to the manufacturer, and glass of the same composition as that of any particular jar is readily to be obtained. The diameter of the bulbs is about 9·5 centims. and their thickness about 0·25 centim. A jar is filled with water nearly to the top of the stem. A wet cloth covering all parts of the bulb and stem below the level of the insulator forms an outside coating. After being sealed, the jars are placed in running water, the temperature of which is never greater than 50° F., nor less than 46° F. The following observations have been made:—

Jar No. 4. Charged 994. Opened after 10 days. No charge remaining.

„ No. 9. Charged 2085. Opened after 7 days 6 hours. The charge remaining was 276.

„ No. 19. Charged 1933. Opened after 10 days. The charge remaining was 868.

The arrangement shown in fig. 2 was employed to determine the relation between temperature and electric conductivity in a flint-glass jar. The jar was filled to the height A, at ordinary temperature, with sulphuric acid. A is a cork supporting the glass insulator C, with the lead trough D containing pumice moistened with sulphuric acid. A piece of platinum wire hangs into the sulphuric acid and terminates in a loop at B, so that the inside coating of the jar is insulated. A stiff wire, M K, soldered to the electrode of the guard-ring electrometer, passes through a small hole at K without touching the glass insulator. Contact between B and this wire may be made or unmade by a small motion of the apparatus. E is surrounded by water. L is a thermometer, J a stirrer. I supplies water when necessary. G is connected with the outside of the electrometer.

To test the cleanness of the glass stem from A to B, a charge was given to the jar by the wire K M. After a certain time no further diminution of charge was observable during ten minutes, and the electrometer-wire was removed, the hole K being closed. The charge was given on Friday. On Monday it was found that less than 20 per cent. had been lost, a proof that the stem was clean, and that the insulation-resistance of the sides of the bulb was very considerable.

The first results obtained by the author are given here in preference to those which have since been obtained. They exhibit the joint effects of polarization and true conductivity, and are instructive when examined along with the table of approximately true conductivity given below. The jar was charged by an electrophorus; when polarization had sensibly ceased, a lamp was placed under the vessel.

The change of temperature between successive readings was pretty regular, except when water was poured in. When hot water was poured in the stirrer was kept in rapid motion.

The rapidities of loss are obtained by dividing the Napierian logarithm of the quotient of two charges or readings by the interval in minutes between the two observations. Thus it is roughly assumed that the capacity of the jar is the same at all temperatures.

Time.	Temp. F.	Reading.	Rapidity of loss.	
h m	°			
1 20 P.M.	56½	1184		
25	63	1184	0	
30	69	1184	…………	Poured in hot water after reading.
35	78	1170	…………	" " "
40	94	1154	0·0023	
45	98	1149		
50	103	1132		
55	107	1126		
2 0	110	1086	0·0037	
5	116	1080		
10	120	1053		
15	124	1036		
20	128	1011		
25	131	982	0·0055	
30	134	950		
35	138	924		
40	140	894		
45	143	861		
50	144	828	0·0099	
55	146	781	…………	Poured in hot water after reading.
3 0	147½	740	…………	Lamp taken away after reading.
5	147½	712		
10	150	690		
15	151	658		
				Charged again.
45½	181·5	1330		
47½	184·2	1137		
50	193·5	936		
52½	201·7	707	0·0120	
55	206·7	495		
56	205·5	443		
57	208	375	0·2776	
58	211·5	319		
				Charged again.
4 5	206	953		
6	206·5	614		
6½	211	518	0·2128	
7½	207	446		
				Charged again.
10½	205·5	1070		
12½	205·5	764		
13½	205	648		
14½	203·5	550		
15½	195	482	0·1982	
16½	192	442		
17½	190·5	422		
				Charged again.
22½	185·5	978		
23½	184·5	905		
24½	184	837		
25½	171	755	0·0391	
26½	170·2	726		
29	159·7	678	…………	Poured in cold water after reading.
30	159	662		

The polarization which occurs after charging is very marked. Some of the later tables of results seem to indicate an increased polarization due to increase of temperature. Thus, when at any low temperature the conductivity calculated from successive intervals is nearly constant, if the temperature is rapidly raised and then kept constant, the conductivity at the new temperature diminishes for a short time as if the jar had just been charged.

[In such a jar the charge is approximately represented by

$$\frac{\mathrm{I\,A\,V}}{4\pi a},$$

where I is the specific inductive capacity of the glass, V the reading of the electrometer, and a the thickness of the glass. But if k is the specific conductivity of the glass, the rate of conduction through it is

$$k\mathrm{A}\frac{\mathrm{V}}{a},$$

and the quantity conducted through, divided by the charge, is equal to

$$\frac{4\pi k}{\mathrm{I}}.$$

Hence

$$\frac{k}{\mathrm{I}}=\frac{1}{4\pi}\cdot\frac{\text{difference of Napierian logarithms}}{\text{difference of times}}.$$

Thus the rapidity of loss given above multiplied by the specific inductive capacity, and divided by 4π, represents the specific conductivity of the glass.

To determine the following Table of conductivities as measured by rapidity of loss, the jar was kept long enough at each temperature for the polarization to become insensible.

Temp. F.	58°	86°	148°	166°	188°	202°	210°
Rapidity of loss	0·000	0·004	0·021	0·025	0·051	0·075	0·084
Rapidity as corrected by a freehand curve	0·000	0·004	0·018	0·029	0·051	0·073	0·090
Rapidity as given by the formula........................	0·000	0·003	0·018	0·029	0·051	0·072	0·091

July 13.]

The free curve drawn to correct the observed conductivities was found to approximate very closely to the logarithmic curve

$$\mathrm{C}=c\,a^{t},$$

where C is the conductivity at the temperature $166+t$, c is the conductivity given by the free curve for 166° F., and a is 1·027.

Messrs. Bright and Clark found that the conductivity of gutta percha followed a similar law.

The author hopes to obtain some definite results with regard to polarization, and to determine for flint glass the law of temperature and absolute specific resistance.

VIII. "Effects of Stress on Inductive Magnetism in Soft Iron." (Preliminary Notice.) By Prof. Sir WILLIAM THOMSON, F.R.S. Received June 10, 1875.

1. At the last ordinary meeting of the Royal Society (May 27), after fully describing experiments by which I had found certain remarkable effects of stress on inductive and retained magnetism in steel and soft iron, I briefly referred to seeming anomalies presented by soft iron which had much perplexed me since the 23rd of December. Differences presented by the different specimens of soft-iron wire which I tried complicated the question very much; but one of them, the softest of all, a wire specially made by Messrs. Richard Johnson and Nephew, of Manchester, for this investigation, through the kindness of Mr. William H. Johnson, gave a result standing clearly out from the general confusion, and pointing the way to further experiments, by which, within the fortnight which has intervened since my former communication, I have arrived at a complete explanation of all that had formerly seemed anomalous. These experiments have been performed in the Physical Laboratory of the University of Glasgow by Mr. Andrew Gray and Mr. Thomas Gray, according to instructions which, in my absence, I have sent them from day to day by post and telegraph.

2. The guiding result (described near the end of my former paper, and referred to in the last paragraph but one of the Abstract in Proceedings of the Royal Society for May 27) was, that the softest wire, tried with weights on and off repeatedly, after it had been magnetized in either direction by making the current, in the positive or negative direction, and stopping it, gave effects on the ballistic galvanometer which proved a shaking out of residual magnetism by the first two or three ons and offs, and a gradual settlement into a condition in which the effect of "on" was an *augmentation*, and the effect of "off" a diminution, of the inductive magnetization due to the vertical component of the earth's magnetizing force. When a fresh piece of the same wire was put into the apparatus and tested with weights on and off it gave this same effect. If the wire had been turned upper end down and tried again in the course of any of the experiments, still this same effect would have been shown. It seemed perfectly clear that in these experiments there was no other efficient dipolar quality of the apparatus by which the positive throw of the ballistic galvanometer could be given by putting on the weight, and the negative throw by taking it off, than the vertical component of the earth's magnetic force.

3. Yet I did not consider that I had *explained* the result by the terrestrial influence, because, for *all* the specimens of steel and soft iron, the effect of weights on had been uniformly to *diminish*, and of weights off to *augment* the magnetism when the magnetizing current was kept flowing. And I was, moreover, perplexed by the magnitude of the

result—the effect of weights on and off shown by the very soft iron wire, under only the feeble magnetizing influence of the earth, being many times (from three times to nine or ten times) as great as the effects which the same weights on and off produced in the same wires when under vastly greater magnetizing forces of the currents through the helix.

4. But by reducing the strength of the magnetizing current gradually, it was clear that the small positive effect of the "on" with the positive current flowing and the small negative effect with the negative current must be gradually brought to approximate more and more nearly to the large positive effect of the "on" when there is no current at all. Immediately after my former communication I therefore arranged to have experiments made with different measured strengths of current, feebler and feebler, until the law of the continuity thus pointed out should be ascertained; and so speedily arrived at the following astonishing conclusions:—

5. (1) When the magnetizing force does not exceed a certain critical value the alternate effects of *pull* and *relaxation* are respectively to augment and diminish the induced magnetization.

(2) When the magnetizing force exceeds the critical value the effects are—pull diminishes, relaxation augments, the induced magnetization.

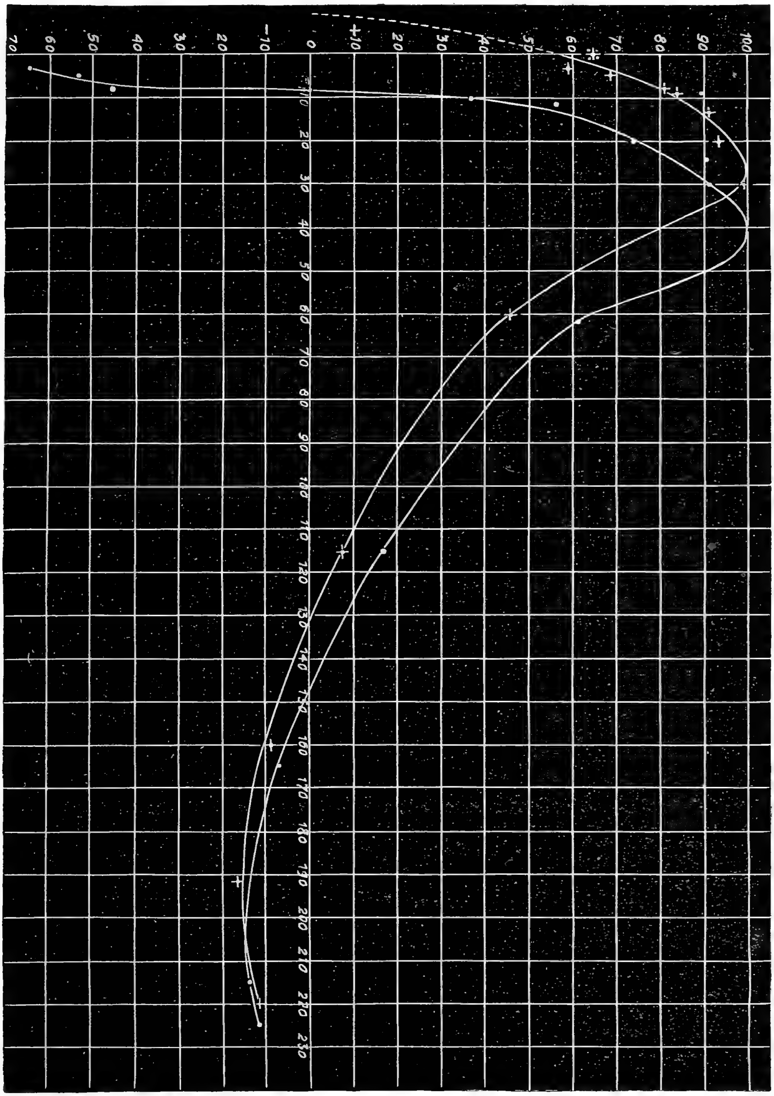
(3) The critical value of the magnetizing force for the annealed Johnson soft-iron wire, with 14 lbs. on and off, is about 17 or 18, if (for a moment) we take as unity the vertical component of the terrestrial magnetic force at Glasgow.

(4) The maximum positive effect of the pull on the inductive magnetism is obtained when the magnetizing force is about 4.

(5) The positive effect of the pull when the magnetizing force is 3 is about eight or nine times the amount of the negative effect when the magnetizing force is 25.

6. The actual results of the experiments which proved these conclusions are exhibited graphically in the accompanying diagram. The horizontal scale (abscissas) shows the numbers of divisions of the scale of the steady-current galvanometer (called for brevity the "battery-galvanometer") used to measure the strengths of the current through the helix. The scale of ordinates shows the numbers of divisions of the scale of the ballistic galvanometer by which the sudden changes of the magnetism of the wire produced by 14 lbs. "on" and 14 lbs. "off" were measured. The ordinates are drawn in the positive direction when the effect of "on" is to increase and of "off" to diminish the magnetism. The simple round spots show the results of observations with currents in the direction called negative (being those which gave negative deflections of the battery-galvanometer). The spots in the centre of signs (+) show results obtained with currents in the direction called positive. The star (*) at the position 64 on the line of ordinates through the zero of abscissas

shows the mean effect of many ons and offs with no current flowing—that is to say, when the sole magnetizing force is the vertical component



of the earth's magnetic force. The curves are drawn as smoothly as may be by hand, one of them to pass as nearly as it can (without intolerable roughness) through all the crossed (plus) dots and the star at 64, the other through all the plain dots. The latter curve cuts the line of abscissas at 8, this being the result (telegraphed to me this evening) of special

experiments made to-day for the purpose of finding accurately the amount of the negative current which, by neutralizing the vertical force of the earth or the wire, gives an accurate zero effect for the "off" and "on." The dotted prolongation of the curve through the plus's, to cut the line of abscissas on its negative side, is ideal, and is inserted to illustrate the relation of this curve to the other. By the two curves cutting the line of abscissas at $+8$ and -8, we see that 8 is the strength of the current, measured on the scale of the battery-galvanometer, which gives a magnetic force in the axis of the helix equal to the vertical component of the terrestrial magnetic force.

7. Next a series of experiments to test the inductive effects of repeatedly making the current always in one direction, and stopping it, with the weight of 14 lbs. always on, and again with the weight off, and this with various degrees of current, feebler than those used in the earlier experiments. The results with all the different intensities of magnetizing force thus applied were the same in kind as that which I found on the 23rd of December, operating with a much stronger magnetizing force on the first soft-iron wire tried; that is to say (contrarily to what I had found in the steel wires), *the change of magnetization produced by repeated applications and annullings of the magnetizing force of the helix was greater with the weight off than on.*

[*Note on Diagram, added July* 2, 1875.—A continuation of the experiments with higher and higher magnetizing powers, since the communication of this paper, disproves the negative minimum indicated by the curves on the diagram, and proves an asymptotic approach to a value approximately -12, of ordinates, for infinitely great positive values of the abscissas.]

Presents received, May 13, 1875.

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"On the Liquation, Fusibility, and Density of certain Alloys of Silver and Copper." By W. Chandler Roberts, Chemist of the Mint. Communicated by Dr. Percy, F.R.S. Received March 11, 1875*.

Alloys of silver and copper possess many curious chemical and physical properties which make them interesting apart from their economic value, and entitle them to careful investigation. The most remarkable of these is a molecular mobility, in virtue of which certain combinations of the constituents of a molten alloy become segregated from the mass, the homogeneous character of which is thereby destroyed.

These irregularities of composition have long been known. Some observations of Lazarus Erckern, in a work published in the seventeenth century†, show that he was familiar with them; that Jars possessed more accurate information on the subject is proved by his stating explicitly, in a memoir published in 1781‡, that in ingots of silver-copper alloys containing much of the base metal, the centre of the mass is less rich than the external portions.

* Read March 18, 1875. See *antè*, p. 349.

† His work was translated in 1686 by Sir John Pettus. See 'Fleta Minor,' Book I. p. 62.

‡ Voyages Métallurgiques, t. iii. p. 270.

The alloys in question have, during the present century, been the subject of many excellent researches in this country and on the continent, the earliest systematic experiments being those of D'Arcet, Inspecteur Général des Essais at the French Mint, who in 1824 investigated the phenomena which attend the cooling of molten mixtures of silver and copper. He does not appear to have published his results; but in 1852 Levol stated, in a well-known memoir*, that the object D'Arcet had in view was the discovery of a method of rendering homogeneous the bars of metal used in coinage. He adds that the researches in this direction offered little prospect of obtaining such a result. I shall presently show, as the result of my experiments, that this conclusion may now be modified.

Levol, in his own experiments, cast the alloy to be examined either in a cubical iron mould of 45 millims. side, or in a sphere 50 millims. in diameter. He concluded that the only homogeneous alloy contains 718·93 parts of silver and 281·07 of copper in 1000; and he considers this to be a definite combination of the two metals, having the formula $Ag_3\,Cu_4$ (or $Ag_3\,Cu_2$ if 63·34 be taken to be the equivalent of copper). All other alloys of silver and copper he views as mixtures of this definite alloy with excess of either of the metals.

In 1860† Matthiessen studied these alloys with the minute accuracy which characterized all his work, and he described them as "mechanical mixtures of allotropic modifications of the two metals in each other." The curve of electric conductivity led him to doubt Levol's conclusion that they were mixtures of the definite alloy $Ag_3\,Cu_2$ (new notation) with silver or copper; for, as he observes, if this were the case, we should expect to find that the curve consisted of two straight lines, connecting Levol's alloy with pure silver and copper respectively. But on examining the curve, starting from the copper side of it, we see that a rapid decrement in conducting-power is caused by copper being alloyed with a small quantity of silver. From the alloy containing 10 per cent. of silver to that containing 65 per cent. we have a straight line; and we may regard the intermediate alloys as mixtures or solutions of these two alloys. Again, from the latter of these to that containing 72 per cent. of silver, we may have a mixture or solution of alloys containing 65 and 72 per cent. respectively. This last point (which corresponds to a conducting-power of 63·7, fine silver being taken as 100) is the minimum point of the curve, and corresponds almost exactly to Levol's $Ag_3\,Cu_2$ alloy. The alloys intermediate between this and pure silver may be mixtures, or perhaps solutions of it in silver containing a small quantity of copper. Now it would appear as if there was an alloy of constant composition at about the point which represents the one containing 65 per cent.

* "Sur les alliages considérés sous le rapport de leur composition chimique," Ann. Chim. et Phys. (3) t. xxxvi.

† Phil. Trans. 1860, p. 173.

of silver; otherwise we should expect a straight line from the alloy containing 72 per cent. of silver to that which contains only 10 per cent.

I now proceed to give the results of my own experiments.

In commencing the inquiry, it seemed probable that, by determining the melting-points of a series of the alloys of silver and copper, information of much interest might be gained as to the arrangement which attends the solidification of a fluid mass of these metals. I adopted a modification of the plan described by Pouillet*, and employed by him in determining the specific heat of platinum at high temperatures.

As soon as the alloy under examination was melted, a wrought-iron cylinder of known weight was dropped into it by means of a wire support. The crucible was then removed from the furnace, and, when the alloy showed signs of solidifying, the iron was transferred to a calorimeter, which consisted of two concentric vessels of thin polished brass, such as is ordinarily used for determining specific heats by the method of mixtures.

It was necessary to determine the mean specific heat of the iron employed, between 0° C. and a known fixed point near the maximum temperature likely to be attained in the course of the experiments. The melting-point of silver was a convenient one, and it has been accurately ascertained by M. Becquerel†, who placed a wire of pure silver in a crucible which was enclosed in a porcelain tube surrounded by the vapour of boiling zinc, the temperature of which has been fixed by M. Deville at 1040° C.‡ As the heat was sufficient to partially fuse the silver, this temperature may safely be taken as the melting-point of the metal.

In order, therefore, to determine the specific heat of the iron, I plunged the cylinder into molten silver, and transferred it to the calorimeter. I may here observe that the film of oxide which formed on the surface of the iron to a great extent protected it from being attacked by the molten alloy; but it was impossible to avoid carrying into the calorimeter a small quantity of metal which adhered to the iron. The metal so introduced was always collected and allowed for. With pure silver 0·05701 was taken as the specific heat, while in the case of alloys the necessary correction was made by deducing the specific heat of each alloy from the specific heats of its constituents; and the equivalent weight of iron was calculated by multiplying the weight of introduced metal by its specific heat, and dividing this product by the specific heat of iron as ascertained by preliminary experiments. This weight was then added to that of the iron employed.

The specific heats of metals at high temperatures have not been deter-

* Éléments de Physique, sixième édition, t. ii. p. 564.

† Ann. Chim. et Phys. (3) t. lxviii. p. 74.

‡ Comptes Rendus, t. lvii. p. 897.

mined, and the adoption of Regnault's numbers in calculating the heat carried into the calorimeter by the alloys may tend to make the results a few degrees too high.

The results of the experiments were calculated by means of the following formula :—

$$x=\frac{(P+p_{,}c_{,}+p_{,,}c_{,,})(\Theta-t)}{p(T-\Theta)},$$

where p is the weight of the iron employed.

P " " water.

$p_{,}c_{,}$ and $p_{,,}c_{,,}$ are the water-equivalents of the calorimeter and thermometer respectively.

T is the initial temperature of the iron.

t " " " " water.

Θ " final "

x " specific heat required.

In one experiment these quantities had the following values :—

p	$=$ 83·140 grms.	T	$=$1040° C.
P	$=$260·520 "	t	$=$ 16° C.
$p_{,}c_{,}+p_{,,}c_{,,}=$	15·687 "	Θ	$=$ 63° C.

The weight of silver carried over was 3·266 grms., the heating effect of which is equivalent to that of 1·306 grm. of iron. Therefore the corrected value of p is 83·140+1·306=84·446 grms.

Substituting these values in the above equation,

$$x=\frac{(260{\cdot}52+15{\cdot}687)(63-16)}{84{\cdot}446\,(1040-63)}$$
$$={\cdot}15734.$$

Three successive experiments gave

·15795,
·15550,
·15734,
the mean ·15693 being finally adopted*.

It may be pointed out that the specific heat of iron as thus determined includes and neutralizes several errors which are incidental to this method of determining high temperatures. The principal of these are :—(1) the loss of heat, which is rendered latent by the small amount of water which is evaporated; (2) the slight difference between the specific heat of the iron and the specific heat of the oxide formed on its surface; (3) the loss

* Weinhold gives 0·1567 as the mean specific heat of wrought iron between 0° and 900° C. (Pogg. Ann. vol. cxlix. p. 214).

of heat sustained by the iron during its transfer from the crucible to the calorimeter; and (4) the radiation from this instrument.

The melting-point of copper has not been exactly ascertained; and I experienced great difficulty in determining it by means of the calorimeter, as the molten metal adheres tenaciously to the iron. Accuracy on this point is not absolutely essential to this inquiry, and I therefore adopted 1330° C., as this is considered by Dr. Van Riemsdijk* to be the probable melting-point of pure copper.

The several alloys were synthetically prepared by melting together pure silver and pure copper; and as soon as the crucible containing the fused metal was withdrawn from the furnace, a small portion of the thoroughly stirred alloy was granulated and set aside for analysis.

The requisite data for ascertaining the melting-point of each alloy were furnished by an experiment similar to that which was made for determining the specific heat of the iron, and in calculating the result it was only necessary to transpose the equation already given, T being the unknown quantity instead of x. The formula then becomes

$$T=\frac{(P+p_{,}c_{,}+p_{,,}c_{,,})(\Theta-t)}{p\,x}+\Theta,$$

the value assigned to x being in all cases 0·15693, the mean specific heat of iron, as given above.

To take an example. In one experiment to determine the melting-point of the 820·7 alloy, the following values were obtained:—

$$\begin{array}{llcl} P & =247{\cdot}74 \text{ grms.} \qquad & t & = 15^\circ \text{ C.} \\ p_{,}c_{,}+p_{,,}c_{,,} & = 15{\cdot}687 \;\;,, & \Theta & = 56^\circ \text{ C.} \\ p & = 82{\cdot}55 \;\;,, & & \end{array}$$

The weight of alloy carried over was 3·608 grms., the heating effect of which was equivalent to that of 1·543 grm. of iron. Therefore the corrected value of p is

$$82{\cdot}55 + 1{\cdot}543 \text{ grm.} = 84{\cdot}093 \text{ grms.}$$

Substituting these values in the above equation,

$$T=\frac{(247{\cdot}74+15{\cdot}687)(56-15)}{84{\cdot}093\times{\cdot}15693}+56$$
$$=874^\circ{\cdot}42 \text{ C.}$$

The results of the experiments are given in the following Table:—

* Archives Néerlandaises, t. iii. (1868).

Melting-points of Silver-Copper Alloys.

No.	Parts of pure silver in 1000 parts of the alloy.	Approximate formula.	Melting-points, in degrees Centigrade.	
			Observed.	Mean.
1.	1000 (pure silver).	...	...	1040
2.	925	$Ag_7 Cu$	919·9 939·0 934·5	931·1
3.	820·7	$Ag_3 Cu$	874·6 891·8 900·5 877·8	886·2
4.*	798	$Ag_5 Cu_2$	882·4 885·4 889·5 890·9	887·0
5.*	773·6	$Ag_2 Cu$	854·9 857·9 862·3	858·3
6.*	750·3	$Ag_7 Cu_4$	852·3 848·5	850·4
7.	718·93	$Ag_3 Cu_2$	868·4 863·5 879·5	870·5
8.	630·29	$Ag Cu$	851·9 844·9 837·6 852·7	846·8
9.	600	$Ag_7 Cu_8$	854·9 849·8 858·6 864·6	857·0
10.*	569·6	$Ag_7 Cu_9$	897·6 902·2	899·9
11.*	561·1	$Ag_3 Cu_4$	910·8 914·8 927·2	917·6
12.*	540·8	$Ag_{20} Cu_{29}$	914·1 916·0 921·5 927·6	919·8
13.*	500	$Ag_3 Cu_5$	931·9 944·1 945·6	940·8

Melting-points of Silver-Copper Alloys (*continued*).

No.	Parts of pure silver in 1000 parts of the alloy.	Approximate formula.	Melting-points, in degrees Centigrade.	
			Observed.	Mean.
14.	497	$Ag_{15} Cu_{26}$	940·2 973·0 981·5 955·6	962·6
15.*	459·4	$Ag\ Cu_2$	953·5 963·9 964·1	960·8
16.	250·5	$Ag\ Cu_5$	1080·8 1141·8 1114·9 1119·1	1114·1
17.	0 (pure copper).	...	...	1330

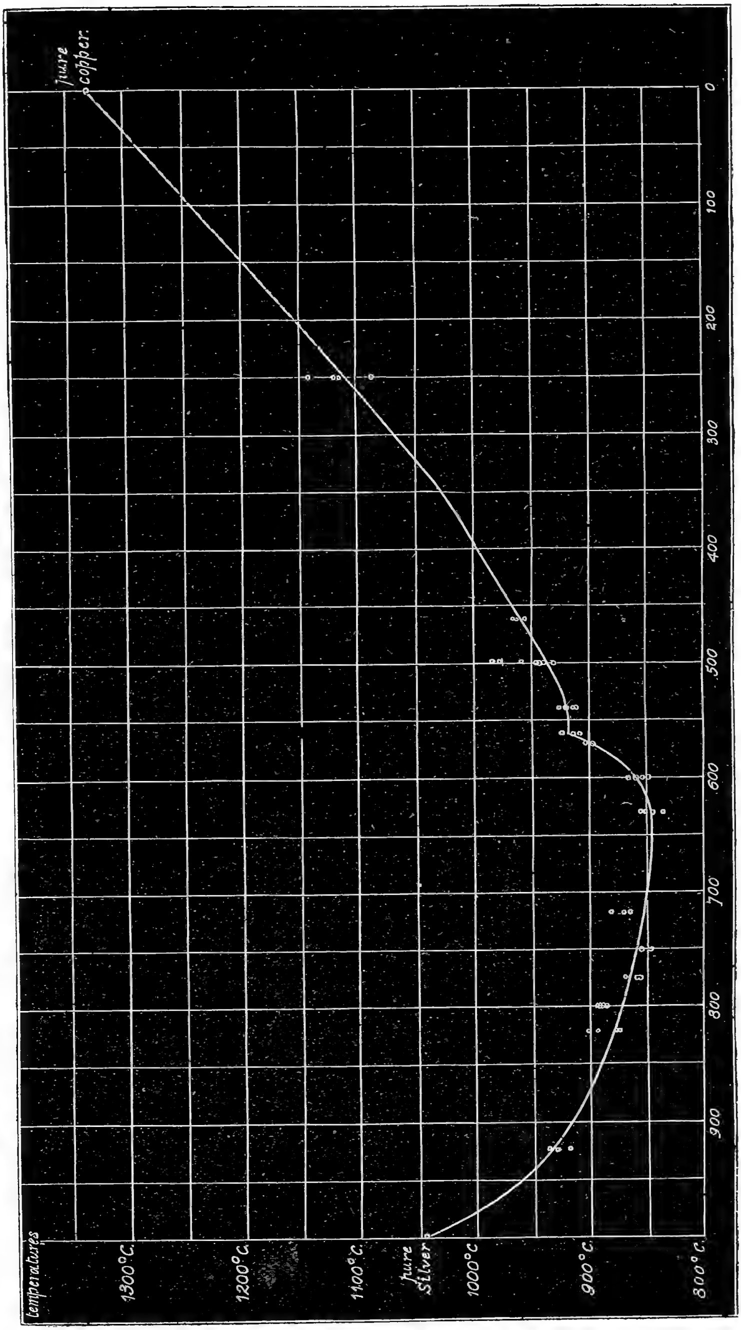
These melting-points are graphically represented by the accompanying curve (next page); the coordinates are the amounts of silver in the several alloys and their melting-points.

It will be observed that the curve exhibits a somewhat rapid decrement from pure silver to the alloy which contains 925 parts of silver, the one employed for the British silver coins. In it the relation between the amounts of metals present is approximately represented by the formula $Ag_7\ Cu$.

The alloys numbered 7 and 8 are of singular interest. The first, which contains 718·93 parts of silver, is Levol's homogeneous alloy; and I anticipated that it would have the lowest melting-point; but the results showed that the alloy containing 630·29 parts of silver (No. 8) melts at a point which is 23·7 degrees lower. In this alloy a very simple relation exists between the atoms of the constituent metals, the formula being $Ag\,Cu$. Additional interest is imparted to it by Matthiessen's curve of electric conductivity having shown that the arrangement of an alloy of this composition would probably be peculiar. From this point the curve passes through the points representing alloys in which base metal predominates to 1330° C., the melting-point of pure copper.

Further evidence as to the melting-points of Nos. 7 and 8 was afforded by placing strips of them in small covered crucibles surrounded by the vapour of boiling cadmium, the temperature of which has been fixed by Deville at 860° C. Both alloys melted, the first partially, the second completely. I am convinced, therefore, that the melting-points of the alloys generally are not inaccurately indicated by the curve. It is, however, not improbable that the examination of a more extended series of alloys may

MELTING-POINTS OF SILVER-COPPER ALLOYS.



Parts of Silver in 1000 parts of Alloy.

point to the necessity of slightly modifying its form. This critical examination is especially necessary in the region of the 497 alloy; for not only do the results obtained on it diverge widely among themselves, but their mean is far removed from the probable line of the curve.

I am not satisfied with the results I have obtained on an alloy which contains 773·2 parts of silver. This alloy is of special interest; its formula is $Ag_2{}' Cu''$, silver being monatomic.

[Since the above was submitted to the Royal Society, I have made additional experiments on alloys in these two portions of the curve. The calorimeter used was of thin polished silver, capable of holding 1200 grammes of water, which were never raised through more than 15° C. The water-equivalent of the instrument was only 15·72 grammes. The masses of iron used were such as had been employed as carriers of heat in the first experiments: the mean of several very concordant results gave ·15003 as the specific heat of the iron when this new calorimeter is employed; and, as has already been pointed out (p. 484), this number includes and neutralizes several errors.

The results are distinguished by an asterisk in the Table, and have been added to those originally indicated in the Diagram. They confirm the direction originally given to the curve in the region of the alloys which contain from 718 to 800 parts of silver; but the existence of a cusp has been detected at the point which represents the alloy No. 11 ($Ag_3 Cu_4$). It may be interesting to point out that the results from which Matthiessen's curve of electric conductivity was developed appear to prove the presence of a cusp at the point which represents the alloy 459·4 ($Ag Cu_2$).—15*th May*, 1875.]

It may be useful to compare these results with those obtained by Rudberg on alloys of lead and tin. He found that when a thermometer is placed in a molten alloy of these metals two distinct stationary points are indicated during the passage from the liquid to the solid state. One of these points is always 187° C.; and in the alloy $Pb Sn_6$ the two points coincide at this temperature—a fact which led Rudberg to conclude that it was the only alloy in which the whole of the metals were chemically combined. I hope, in continuing this inquiry, to be able to ascertain whether the change of state in the case of silver-copper alloys also terminates at a constant temperature. I may mention that M. A. Rich* determined the melting-points of certain alloys of tin and copper by means of Becquerel's thermo-electric pyrometer; and he obtained concordant results with the alloys $Sn Cu_3$ and $Sn Cu_4$; but with all other alloys the results differed widely among themselves.

It is at present difficult to show the direct bearing of these results on the phenomena of liquation in alloys of silver and copper; but the curve is valuable, as it proves that the alloys Nos. 7 and 8 occupy positions in

* Ann. Chim. et Phys. t. xxx. p. 351.

LIQUATION IN SILVER-

ALLOY CONTAINING { 925 SILVER. 75 COPPER.

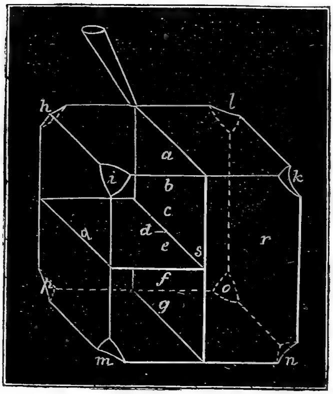


Fig. 1.

RAPIDLY COOLED.

Vertical Plane.	*a.*	924·6
	b.	926·0
	c.	929·1
	d.	935·5
	e.	931·0
	f.	925·0
	g.	924·2
Corners.	*h.*	923·2
	i.	923·7
	k.	923·3
	l.	923·3
Corners.	*m.*	923·9
	n.	923·8
	o.	922·7
	p.	923·2
Sides.	*q.*	923·6
	r.	923·8
	s.	923·1

"Dip assay," 925·1.

Maximum difference [between the centre and the corners], 12·8 per thousand.

ALLOY CONTAINING { 900 SILVER. 100 COPPER.

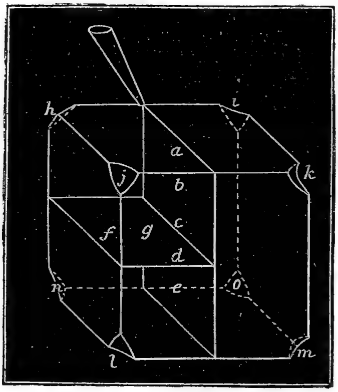


Fig. 3.

SLOWLY COOLED.

Vertical Plane.	*a.*	899·0
	b.	899·2
	c.	899·6
	d.	898·6
	e.	899·1
Corners.	*h.*	898·9
	i.	898·6
	j.	899·4
	k.	898·3
Corners.	*l.*	899·6
	m.	899·5
	n.	899·3
	o.	898·8
Horizontal Plane.	*f.*	898·5
	g.	898·6

"Dip assay," 900·4.

Maximum difference, 1·3 per thousand.

ALLOY CONTAINING { 630·3 SILVER. 369·7 COPPER.

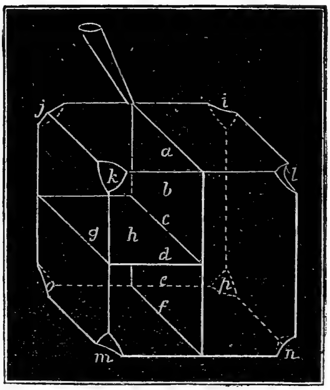


Fig. 5

SLOWLY COOLED.

Vertical Plane.	*a.*	614·9
	b.	631·5
	c.	636·0
	d.	635·0
	e.	630·9
	f.	634·8
Corners.	*i.*	620·6
	j.	620·3
	k.	625·9
	l.	625·0
Corners.	*m.*	632·6
	n.	633·5
	o.	633·9
	p.	634·
Horizontal Plane.	*g.*	631·4
	h.	635·9

"Dip assay," 630·2.

COPPER ALLOYS.

ALLOY CONTAINING { 925 SILVER. 75 COPPER.

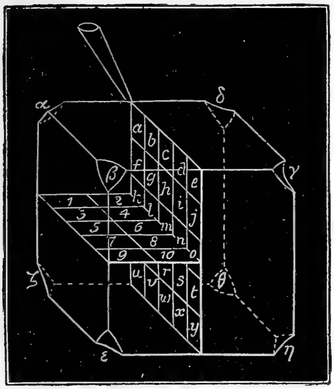


Fig. 2.

SLOWLY COOLED.

Vertical Plane.	
a.	925·7
b.	925·0
c.	925·0
d.	925·0
e.	925·4
f.	924·3
g.	925·0
h.	925·3
i.	925·3
j.	925·3
k.	924·3
l.	925·3
m.	925·3
n.	924·4
o.	925·0
p.	924·3
q.	925·0
r.	925·3
s.	925·0
t.	924·9
u.	924·3
v.	924·7
w.	924·9
x.	924·9
y.	925·3

Horizontal Plane.	
1.	924·8
2.	925·0
3.	924·9
4.	924·9
5.	925·0
6.	925·1
7.	925·1
8.	925·1
9.	925·0
10.	925·0

Corners.	
α.	924·1
β.	924·1
γ.	924·1
δ.	924·4
ε.	924·0
ζ.	924·2
η.	924·2
θ.	923·9

"Dip assay," 924·9.

Maximum difference [between the centre and the corners], 1·40 per thousand.

ALLOY CONTAINING { 718·93 SILVER. 281·07 COPPER.

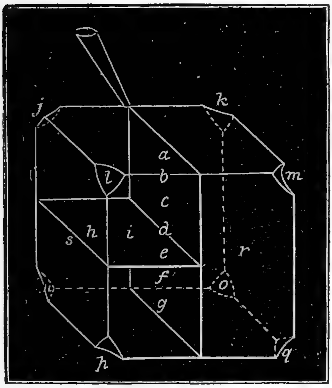


Fig. 4.

SLOWLY COOLED.

Vertical Plane.	
a.	718·3
b.	719 5
c.	718·3
d.	718·4
e.	718·3
f.	718·4
g.	718·2

Horizontal Plane.	
h.	718·7
i.	718·5

Sides.	
r.	718·8
s.	718·4

Corners.	
j.	719·0
k.	719·0
l.	719·0
m.	719·4
n.	719·1
o.	719·0
p.	719·0
q.	719·1

"Dip assay," 719·0.

Maximum difference, 1·2 per thousand.

ALLOY CONTAINING { 333·3 SILVER. 666·7 COPPER.

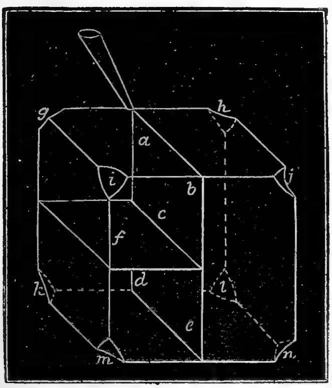


Fig. 6.

SLOWLY COOLED.

Vertical Plane.	
a.	342·8
b.	333·0
c.	337·5
d.	330·0
e.	332·0

Corners.	
k.	331·0
l.	334·0
m.	336·3
n.	334·4

Corners.	
g.	331·0
h.	332·0
i.	331·5
j.	334·8

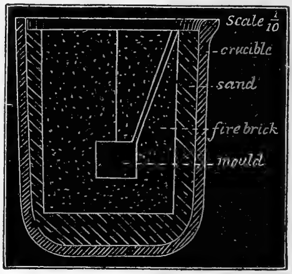
Horizontal Plane.	
f.	336·2

"Dip assay," 333·4.

the lower portions of the curve similar to those which they hold on Matthiessen's curve of electric conductivity.

The range of temperature which these melting-points exhibit appears to justify the conclusion that liquation is in some way the result of the unequal cooling of a mass of silver and copper, and that if the cooling could be greatly protracted the liquation would be considerably modified. In order to ascertain whether this were the case, I used cubical moulds (about 45 millims. side) of firebrick (fig. 7), which were easily heated to bright redness, and in which the alloys could be *slowly* and *uniformly* cooled*.

Fig. 7.



All the cubes represented on pp. 490 & 491 were cast in moulds of this description. The first of these (fig. 1), the composition of which was about 925 parts of silver per 1000 of the alloy, was cooled rapidly. Its structure confirms Levol's general conclusion, as the centre contains 12·8 parts per thousand more silver than the external portions. On the other hand, fig. 2 shows that when the same alloy is slowly cooled the constituents hardly undergo any molecular re-arrangement, the maximum difference being only 1·4 per thousand. A cube of the alloy used for the French coinage, when rapidly cooled, exhibits a difference of 10·1 parts per thousand between the centre and the corners; but when the same metal is slowly cooled (fig. 3), the variation is only 1·3 parts per thousand. The maximum difference found by Levol in his homogeneous alloy was 0·44 part per thousand. I find (fig. 4) that if the cooling is slowly effected this homogeneity is disturbed, the external portions being slightly richer in silver than the centre. Fig. 5 exhibits the results of an experiment on the alloy which has the lowest melting-point and the simple chemical formula Ag Cu. Its structure is interesting, as the action of gravity appears to have influenced the arrangement, the lower parts of the cube being richer than the upper. The maximum difference is 21·1 parts per thousand. I should observe that Levol found the corners of a cube of this alloy to be 15 parts richer than the centre; but the only alloy in which he detected any effect of gravity was that containing 690 parts of

* In treating of ingots of low standard, Jars stated in 1781, in the work to which I have already alluded (note, p. 481):—"Je remarquai par des expériences que pour rendre les lingots d'une teneur plus égale dans toutes les parties il falloit que les lingotières fussent aussi chaudes qu'il est possible." I should state that certain unpublished experiments by Dr. Boycott, formerly Assay Master in the Calcutta Mint, have shown that the liquation of silver-copper alloys is modified by casting the metal in sand moulds, and that Mr. E. Seyd suggested in 1871, in a work printed for private circulation, the use of hot iron moulds in casting gold and silver, as an improvement in the process of coinage, the bars being "more equal in temper and in molecular arrangement" (patented in 1872, No. 368).

silver. Fig. 6 shows the results of analyses on the different portions of a mass of the alloy containing 333·3 parts of silver per thousand ($Ag\ Cu_4$). The mass varies in composition, but the arrangement does not appear to have been guided by any law.

The inquiry appears to show that several alloys of silver and copper are, under suitable conditions, as homogeneous as Levol's alloy, the chief peculiarity of which consists in its not being liable to liquation when poured into a mould at the ordinary temperature and cooled rapidly.

It will be remembered that experiments prove that in all alloys which contain less than 71·89 per cent. of silver the external parts are richer than the centre. The curve of fusibility shows that the alloys which contain less than 35 per cent. of silver have higher melting-points than other alloys of silver and copper, or even than pure silver. It would not appear, therefore, that liquation is the falling out of the least fusible alloy present in a mass of silver and copper; for if it were, the external portions of the alloys would in all cases be less rich in silver than the centre.

I cannot at this stage of the inquiry offer a complete explanation of this molecular rearrangement; but I venture to think that the results already obtained are interesting. They show, first, that the same alloys are situated on the turning-points of the curves of fusibility and electric conductivity; and second, that the arrangement of an alloy is to a great extent dependent on the rate at which it is cooled.

In accordance with a suggestion made to me by Mr. R. Mallet, I have endeavoured to determine the relation between the densities of silver in the solid and the molten state. I adopted the method which he devised and has employed in the determination of the density of molten cast iron *.

A conical vessel of best thin Low-Moor plate (1 millim. thick), about 16 centims. in height, and having an internal volume of about 540 cub. centims., was weighed, first empty, and subsequently when filled with distilled water at a known temperature. The necessary data were thus afforded for accurately determining its capacity at the temperature of the air. Molten silver was then poured into it, the temperature at the time of pouring being ascertained by the calorimetric method already described. The precautions, as regards filling, pointed out by Mr. Mallet were adopted; and as soon as the metal was quite cold, the cone with its contents was again weighed.

The surface of the molten metal in the crucible was covered with charcoal; and as pure silver, when in a liquid state, is known to absorb oxygen if exposed to the air, the cone was filled with an atmosphere of coal-gas.

* Proc. Roy. Soc. vol. xxii. p. 366, and vol. xxiii. p. 209.

The most important of the corrections applied to these results was that for change of volume of the iron vessel which attended the introduction of the molten metal.

Different qualities of wrought iron vary considerably as to dilatation by heat. This fact, together with the known increase in the expansion at high temperatures, rendered it necessary to determine the mean coefficient between 0° C. and the melting-point of silver. For this purpose a modification of Ramsden's method was adopted, the iron being placed in a graphite trough and surrounded by molten silver. The micrometer-reading was taken when the length of the iron remained for a short period invariable, as this was the true solidifying-point of the silver, the loss of the latent heat of liquefaction rendering the temperature constant.

A great number of experiments were made; and although they were attended with much difficulty, I believe the following results to be trustworthy (the numbers represent the mean coefficient of linear expansion per 1° C. of the Low-Moor iron employed, up to the temperature of melting silver):—

·00001242,
·00001254,
·00001215,
·00001219,
·00001271,
Mean.... ·00001240,

which gives a mean coefficient of cubical expansion

·00003720.

This result is considerably higher than that of Rinmann, who gives 0·00002808 as the mean coefficient of cubical expansion of wrought iron between 15° C. and "a white or welding heat."

The results of the experiments made with a view to ascertain the densities of pure silver and of Levol's homogeneous alloy when in a molten state are given in the accompanying Table (p. 495). This alloy was chosen for the experiment, as its density when solid very nearly agrees with that calculated from the densities of the constituent metals.

The cubic dilatation was, in the case of pure silver, in the ratio of 9·4612 : 10·57. Deducing from this the mean coefficient through, say, 1050° C., we obtain

0·00011164 per 1° C.

The coefficient of linear expansion was therefore

0·00003721.

The mean of the coefficients of linear dilatation of silver between 0° and 100° C., given by various authorities, is

0·00002015.

It will thus be seen that the expansion of silver between 0° C. and 1050° C. is about twice as much as it would have been had this rate of expansion been maintained through the whole range of temperature.

The mean coefficient of linear dilatation of Levol's alloy, as deduced from the results given in the Table, is

0·00003703 ;

but it is impossible to compare this with the rate of expansion at low temperatures, as the latter has not been ascertained.

	Initial volume of cone.	Volume of cone filled with molten metal.	Temperature of metal when poured.	Weight of metal.	Density when fluid.	Density of solid metal.
Pure silver.	c. c. 536·6	c. c. 556·3	°C. 1143	grms. 5255·4	9·4468	10·57
	542·9	564·4	1223	5348·3	9·4757	
	Mean				9·4612	
Levol's alloy.	735·13	778·06	1020	7062·4	9·0788	9·9045 [Levol], 9·998 by calculation.
	537·42	557·25	1131	5033·4	9·0321	
	Mean				9·0554	

In conclusion I have much pleasure in acknowledging the assistance I have received from one of the Assistant Assayers, Mr. Edward Rigg, whose cooperation has been of much service to me; and I must also express my thanks to Joseph Groves, Senior Fireman, who aided me in the furnace-operations.

Correction to Prof. CAYLEY'S "Eighth Memoir on Quantics." Phil. Trans. Vol. 157 (1867). Received June 26, 1875.

The Table for L, M, L′, M′, p. 544, should stand:—

$72\,L=$		$72\,L'=$	$24\,M'=$
:		$A^3I\ +1$	$I\ -1$
:		$ABI\ +3$	
$A^2B^2C\ -81$		$CI\ -15$	

and substituting these values we find for 36a, 36b, &c. the values given p. 554; where in the expression of 36a, the term A^2B^2C-126 should have been distinguished by an asterisk, to show that there was an alteration in the coefficient, -126, instead of -36 as given p. 544.

June 17, 1875.

JOSEPH DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

Prof. John Casey, Dr. August Dupré, and Dr. James Hector (who was elected in 1866) were admitted into the Society.

The following Papers were read:—

I. "On a new Form of Dynamo-Magneto-Electric Machine." By S. C. TISLEY. Communicated by W. SPOTTISWOODE, M.A., Treas. R.S. Received June 17, 1875.

In the first machines constructed by Siemens and Wheatstone in 1867 (see Royal Society's Transactions) the power of augmenting the magnetism by successive currents, developed from the original residual magnetism contained in the iron, was fully demonstrated, and it was shown that the power of the machine could thereby be developed to a great extent; but the only means for obtaining external work was by the insertion in the circuit of a magnet or coil so that the secondary discharge could be utilized. Sir Charles Wheatstone also showed that a great part of the current could be shunted through a platinum wire, care being taken that the resistance of the platinum wire was sufficient to compel a large part of the current to pass round the electromagnet.

In the same year the writer designed a machine which was made by Mr. Ladd, and described by him in a paper read before this Society (see Transactions), the principle of which was that two separate armatures

being introduced, one was employed for magnetizing the machine, the other being used for external work. This machine gave a good electric light &c., and was shown in the Exhibition of Paris, 1867, when a silver medal was awarded for it.

To simplify this machine, the author of this paper afterwards placed the two armatures in the same groove between the poles of the electro-magnet, bolting the two together at right angles to each other, so that they came under the influence of the magnetism alternately; by this method one pair of bearings was sufficient instead of two, and the machine altogether was much simplified.

The machine now about to be described is a still further modification, in which the greatest amount of simplicity and effective power are combined.

The apparatus consists essentially of an electromagnet with shoes, forming a groove in which a Siemens's armature is made to revolve: this is much the same as the original machines made by Siemens and Wheatstone; but the difference occurs in the break or commutator; here there are two springs or rubbers employed in taking the current off from the commutator. The commutator consists of three rings: one of these rings is complete for three quarters of the circle, the other quarter being cut away; another ring is cut away three quarters, leaving the one quarter; and in between these two rings is a third ring, insulated and connected with the insulated end of the wire wound round the armature; on this centre ring are projecting pieces, one a quarter of a circle and the other three quarters, so arranged as to complete the two outer circles. The rubber spring which comes into contact with the quarter of the middle circle is connected with the electromagnet of the machine, and the armature is so arranged that at the time of contact the best magnetizing current is developed. The other spring rubber is in connexion with the wire on the armature during the other three quarters of its revolution; and this is connected with any external piece of apparatus required to be worked.

By this arrangement, the alternate currents being utilized, they are all in the same direction; and by the length of contact the whole of the current is obtained in the best condition for heating wires, decomposing water, giving an electric light, and other usual experiments.

At present a model machine has been constructed on this principle, the armature of which measures 5 inches long by 2 inches diameter, on which is wound about 50 feet of cotton-covered copper wire, no. 16, B. W. G. The magnet has about 300 feet of covered copper wire, no. 14, B. W. G.: the whole instrument, without the driving-gear, weighs 26 lbs.; with this apparatus 8 inches of platinum wire, ·005, can be made red-hot, water is rapidly decomposed, &c.

The armature is constructed specially to prevent the accumulation of heat to which every class of dynamo-magneto-electric machine is liable. It is

made in two halves, a groove of a zigzag form being cast in each half, so that when the two are screwed together a continuous channel is maintained through the bearings for a current of cold water to pass during the whole time the machine is at work.

The advantages suggested by these arrangements are their extreme simplicity, the few number of parts, only one armature and one wire being used.

This principle of the alternate current being utilized is also applicable to machines constructed on the multiple armature principle; and the economy thereby resulting would prove of great advantage, as the power of the machine could be varied by throwing into the electromagnets either every other current, or every fourth, sixth, or eighth current, according to the strength required in the machine, the whole of the other currents being utilized for electric light or otherwise.

II. "Note on the Anatomy of the Umbilical Cord." By Lawson Tait, F.R.C.S. Communicated by W. S. Savory, F.R.S. Received April 28, 1875.

(Abstract.)

I. Its external form and method of growth.
II. Its covering.
III. Its substance.
IV. Its vessels.
V. Its relations to the fœtus and placenta.
VI. Its nutrition.

I. The spiral form of the cord has received many explanations; but hitherto none has seemed satisfactory, nor sufficient to explain all the facts. The cause of the spiral form has generally been regarded as existing in the arteries; but experiment shows that the vein is the chief factor.

The considerations drawn from the comparative and teratological anatomy of the cord point to the conclusion that its twist must depend upon some mechanism at the fœtal insertion.

Such mechanism is found in a peculiar camb-like growth of the dermal ring of the umbilicus, and in an arrangement of capillaries upon which the nutrition of the cord depends, that nutrition being supplied over the venous surface of the cord in about the proportion of three to two on the arterial surface. This unequal nutrition would seem necessarily to result in a spiral.

II. When the surface of the cord is treated with litmus or hæmatoxylin, the epithelial covering is found to consist of a single layer of irregularly polygonal cells, regularly nucleated. The fibrillar matrix on which they lie is evidently only a slightly condensed arrangement of the canalicular tissue. Silver-staining shows that these cells have a peculiar irregularity in size and arrangement.

Well-marked stomata, both spuria and vera, are to be seen on the surface, the latter unquestionably forming orifices of entrance into the vast system of canals of which the proper system of the cord is composed.

The epithelium varies somewhat in its arrangements near the placenta and near the fœtus.

In the former position the cells are smaller, more irregularly jointed, and apparently somewhat more elongated in the direction of the long axis of the cord than they are near the fœtus.

The whole appearance of the cells gives the impression that the covering is older here; and in the canals and in the stomata are to be seen rows of minute refracting globules, visible only under very high powers, the nature of which I have been unable to make out, as they appear only after deep silver-staining.

III. The alveolated canalicular tissue of the cord is divided throughout its entire length into three columns, the divisions between which are not visible to the naked eye, but become very perceptible when one of the columns is injected by Recklinghausen's method.

When the canals are empty they present the appearance of fibrous tissue by the collapse of their walls, and when partially distended they look like stellate cells. This has led to the erroneous description of a fibrous matrix in which occur stellate cells. In the lacunar spaces of the canals the oval nuclei are imbedded.

These nuclei do not alter their shapes or positions.

In injecting this system of canals, the fluid passes more readily in the direction from the fœtus to the placenta than in the reverse way.

During the process minute streams of the injection may be seen flowing from the surface of the cord; and these are not due to rents.

Transverse and longitudinal sections of the injected columns show that the canals are stellate in every plane.

In the alveoli between the canals the wandering cells are found. Silver-staining shows that these canals are walled. The nuclei are not fusiform, for when magnified 1000 diameters they are seen to be oval and provided with a very small nucleolus. The statement that they send processes into the branches of the canals is due to an optical illusion, dispelled by the use of high-power immersion lenses.

The round cells which occur in the alveoli have very large nuclei, quite disproportionate to the surrounding protoplasm.

They are not constant. In some cords, especially those removed from large children, they are found very scantily, whilst on the cord of a small eight-months' child they were found to be extremely abundant.

They are also often more numerous in some parts of the cord than in others, and in one district of the cord than in another.

They are most abundant near the umbilicus and near the capillaries. They may be seen moving on the warm stage and exhibiting amœboid movements after having taken up litmus colour.

I have not been able to discover any nerve-fibres in the cord.

The canalicular tissue may be demonstrated to end in three cones, one for each column of the cord, the apices of which are just within the dermal ring. The injection will not pass through the tendinous ring; nor have I succeeded in making it enter the capillaries of the cord from the canalicular tissue.

When the injection of the capillary plexus running from the dermal ring has been successfully accomplished (and this is, for many reasons, a difficult thing to do), there will be found a peculiar vascular arrangement in the centre of the cord, lying in the firm nucleated tissue which forms the omphalic ring. The basis of this arrangement is a peculiar sacculated sinus, a mere excavation in the fibrous tissue, as it is doubtful if it has any definite wall. It seems to have a spiral arrangement, for in one section it appears and disappears as only a screw could.

It extends from the omphalic ring at least forty-five millims. up into the true substance of the cord, giving off at short intervals thick trunks which rapidly break up into capillaries.

These capillaries do not form loops, but enter directly into the canalicular tissue; and it is possible to inject a large extent of all three districts of the substance of the cord by passing the injection through the fœtus. This sinus seems to originate from the small arteries of the anterior abdominal wall, which enter with the vein.

There seems to be a close analogy between this arrangement and that of the Haversian system in bones; indeed the actual resemblance sometimes seen is very close.

IV. The proper tissue of the vessels is made up of the ordinary fusiform fibre-cells, with their characteristic rod-shaped oval nuclei. The outer layers seem to have wider limits of contraction than the inner—these latter being thrown into rugose folds on the contraction of the arteries, resembling the appearance of the contracted urethra or œsophagus. One reason of this exceptional range of contraction is, that the fibres of the muscular tissue are arranged in bundles having a double spiral direction. The contraction of the fibres seems to be governed by the blood-current, as they contract as soon as the blood in them becomes arterial by the establishment of respiration; and they may be seen again to relax and pulsate if the blood becomes again venous by the temporary arrest of respiration.

This appears to be aided by the absence of an endothelial lining to the arteries, which my observations seem to establish.

The closure of the vessels is effected immediately by clot, and subsequently by the agency of the round migratory cells.

These wandering cells seem also to share in the subsequent processes of inflammatory ulceration and necrosis of the stump of the cord, and in the removal of the coats of the remaining vessels.

V. The relation of the cord to the fœtus has already been described.

The limitation of the canalicular tissue of the cord, at its placental attachment, is quite as abrupt.

The injection-fluid cannot be made to pass from the substance of the cord into the placenta, for it is arrested by a firm membrane derived from the chorion, which the vessels of the cord penetrate, and between two layers of which they lie. There is absolutely no connexion between the nutritive system of the cord and that of the placenta.

VI. The chief factor in the nutrition of the cord is the arrangement of capillaries entering it from the fœtus. From the facts observed by me in cases of extra-uterine gestation, it is likely, however, that the stomata of the epithelial surfaces of the cord play an important part in its nutrition.

The liquor amnii contains substances which are very suggestive that the fluid is used for purposes of nutrition, and perhaps for the nutrition of the cord. In recent cases of extra-uterine fœtation, before the liquor amnii becomes absorbed, the cord remains fresh and plump. After the fluid has been absorbed the cord becomes shrivelled; but it still retains its structural characters, minus the wandering cells. It may be, therefore, that the canalicular nuclei are able to keep the cord in repair, as it were, by the matters absorbed from the liquor amnii, until that fluid disappears—very much as ivy continues to live after its connexion with its root has been severed. Indeed the analogy between the umbilical cord and vegetable tissue is, as I hope to be able to show further, a very close one.

III. "First Report of the Naturalist accompanying the Transit-of-Venus Expedition to Kerguelen's Island in 1874." (Conclusion.) By the Rev. A. E. Eaton. Communicated by the President. Received May 10, 1875.

In January 1875, shortly after the departure of the American Expedition from Royal Sound, an opportunity occurred of visiting another part of Kerguelen's Island. To relieve the ennui of his officers and men, who by that time were thoroughly tired of being detained without any definite occupation in an uninhabited island, Captain Fairfax ordered the 'Volage' to leave Observatory Bay, and proceeded to Swain's Bay, where he remained three weeks. During this period he entertained me as his guest, took me to the best localities in the bay for collecting, and rendered me every assistance that lay in his power. The Royal Society is therefore indebted to Captain Fairfax for a fine series of Algæ from Swain's Bay, comprising many species not found in Observatory Bay, and some that were not known to be indigenous to the island. Most of these are described in the 'Flora Antarctica' as Falkland-Islands species. Captain Fairfax at the same time enabled me to secure the skeleton of a *Globiocephalus*, which was found dead in shallow water by Mr. Forrest (Mids.). Most of the epidermis had been removed by small

crustacea, so that it was not possible to ascertain the colour of the animal; but Lieut. Goodridge, R.N., very kindly photographed the carcass before it was flensed, and its dimensions were carefully taken by one of the boat's crew, and therefore it will be easily identified.

Young Sea-Elephants were frequently found by us in Swain's Bay. Some examples are uniformly reddish brown, others are pale, blotched and spotted with darker grey. They usually lie just above the beach, separately, in hollows among the *Acœna* and *Azorella*, where they are sheltered from the wind. On being approached they make no attempt to move away (possibly because there are no land animals indigenous to the country capable of molesting them to cause them to acquire a habit of flight), but raise up the fore part of their body, open the mouth wide, and utter a peculiar slobbering cry. My mammalian specimens, unfortunately, are not so complete as they were when first procured, owing to the impossibility of preventing "liberty men" and others taking an interest in such "great curiosities" whilst the process of cleaning them was in progress. The removal of stones, purposely laid upon some of the bones, led to the loss of the fore limbs of seals, &c., which were blown away by the wind.

All of the birds, with the exception of two species (a *Procellaria* and a *Thalassidroma*), are represented in the Cape-Town Museum.

Thalassidroma Wilsoni (Dr. Wyville Thomson, however, seems to consider the Kerguelen-Island bird to be another species) arrived in the Sound in great numbers a few days before the "Transit." Towards the end of January they commenced laying their eggs generally. By the second and third weeks of February the incubation of the eggs was usually far advanced; and a day or two before we left the island, Capt. Fairfax sent me a young bird recently hatched. The tarso-metatarsal joint is not elongated in the chick. I failed to find the eggs of *Thalassidroma melanogaster*; the birds occurred to me only in pairs.

It may be well to explain that Petrels sit in their holes in pairs until the egg is laid. Then usually only one bird is found at a time upon the nest until the young are hatched; and soon after they have issued from the egg the young are found alone during the day. For whilst incubation is in progress, the bird not upon the nest is either asleep in a siding or branch of the burrow or (more commonly) is spending the day at sea; and when the young are a day or two old, both of the parents absent themselves during the day, and only return at night for the purpose of feeding them.

Along the coast, outside Swain's Bay, a few examples of *Diomedea melanophrys*, a species not observed in Royal Sound, were noticed.

In the less frequented parts of the island some of the birds were unusually fearless and tame. Shags would submit to be stroked along the back without getting off their nests or attempting to peck the hand. More than once Sheathbills, and on one occasion a Skua, fed out

of my hand. A Sheathbill, after pecking at my boots, ate in succession six eggs held out to it. But the Skua behaved in a still more extraordinary manner. On approaching within three hundred yards of the nest it was evident, from the excitement of the old birds, that the young were hatched; and on searching for the nestlings, the old birds commenced their usual onslaught when within two hundred yards of the nest. Disregarding their outcries and fierce swooping down, I soon found the young ones crouching amongst the herbage some distance apart from one another and the nest (which they leave at an early age), and sat beside the nearest. The hen Skua immediately alighted within a yard of me and continued her vociferations, whilst the cock withdrew to the other nestling. On stroking her chick the hen became more excited than ever and advanced a little nearer. Taking a Prion's egg from my pocket and holding it out, her cries ceased whilst she eyed the egg, but recommenced when she again looked at me. She once more looked at the egg, became silent, waddled cautiously up and pecked gently at my finger, then, reassured, pecked the egg, which she very soon made an end of. In the same way she ate a young Prion killed for the purpose, and afterwards flew to the hole from whence the bird had been taken to see if it contained another; and upon my digging at some other holes, she came near and stood by in eager expectancy of further gratuities. With regard to her pecking first at the finger before the egg, I would observe that wild birds usually do this previous to feeding out of the hand. The Sheathbills did the same, and so do English birds which have never been in confinement. It seems to be their way of testing the nature of any strange-looking object.

The Sheathbill was plentiful in Swain's Bay, and a fair number of their eggs were procured. As Dr. Kidder, the American Naturalist, had not succeeded in finding any, I was anxious that he should have some; but did not consider myself at liberty to give him more than one, and that a damaged specimen almost in halves. The Royal Society will now be able to be more liberal.

A fine male example of a *Raia*, differing from the species previously found in Royal Sound, was shot by Mr. Budds, the chaplain of H.M.S. 'Volage,' two days before we sailed.

The *Agrostis* mentioned when I last wrote came into flower about the third week in January. It can scarcely be said to form a sward, or pasturage even, in the neighbourhoods visited by me. The *Limosella* was found in February in fruit and flower, very sparingly, in only one shallow lake between the Observatory and Mount Crozier.

I omitted to inform you that the Kerguelen-Island *Callitriche*, given in the 'Flora Antarctica' as *C. verna* var. *terrestris*, should (I think) be regarded as a form of *C. pedunculata* rather than of *C. verna*. It has no bracts, and seems to exhibit other peculiarities of *C. pedunculata*. Prof. Wyville Thomson alludes to it as *C. verna*; but probably he

adopted the name from the 'Flora' without suspicion, unless, indeed (which is unlikely), both species occur on the island. For the satisfaction of other botanists I have brought back specimens of the plants in spirits, showing flower and fruit, as well as dried examples.

The fern, which was new to me, according to Lady Barkly, may be a form of *Polypodium* (*Grammitis*) *australe*.

In the following particulars I am sorry to have occasion to report failure.

The moss-eating Lepidopterous larvæ all died before our arrival at the Cape.

All the larger Algæ collected were spoilt. One suite of dried examples was lost, through the box in which they were contained being placed open, in the rain, by one of the servants a few days before we sailed, without my knowing it had been moved from its place. The second set, gathered the day before we left the island, was sent on board the 'Supply,' with directions that the box should be placed in an accessible position: unfortunately the message miscarried, the box was stowed away in the hold, and I could not get at it until a fortnight afterwards, when almost the whole of its contents were completely decomposed.

Again, series of examples of some of the flowering plants were lost through the difficulty of attending to them when collected.

I left Kerguelen's Island in H.M.S. 'Supply' on the 27th February, arrived at Simon's Bay on the 31st March, and at Gravesend on the evening of the 7th May. In the course of the voyage I collected a few animals and Algæ with the towing-net.

IV. "On the Determination of Verdet's Constant in Absolute Units." By J. E. H. Gordon, B.A., Gonville and Caius College, Cambridge*. Communicated by Professor J. Clerk Maxwell, F.R.S. Received May 5, 1875.

(Abstract.)

In the year 1845 Faraday discovered that certain media possess the property of rotating the plane of polarization of light passing through them when a magnetic force acts on them. About the year 1853 M. Verdet found that with the same magnet and medium the rotation is directly proportional to the strength of the magnet—that is, that the ratio between the amount of rotation and the intensity of the magnetic field is constant.

The object of this investigation is to measure this constant in absolute

* The whole of this work has been done under Prof. Clerk Maxwell's superintendence. He suggested the method and nearly all the details; and any merit which the investigation may have belongs to him. He is, however, in no way responsible for any errors there may be in the numerical results.

units for a standard substance. Distilled water was used, and the magnetic force was produced by means of an electric current in a helix, as the magnetism of iron magnets is an undetermined function of the shape and nature of the iron core.

The strength of the helix was determined by comparing the magnetic force at a series of seven equidistant points along its axis in terms of that at the centre of the great dynamometer of the British Association, whose power is known in absolute measure.

The intensities were compared by varying currents sent opposite ways through each, till the action on a small magnet at their common centre was *nil.*

The intensity at each of a series of points being known for a given current, the difference of magnetic potential at the two ends for that current was obtained by integrating with respect to the length between limits corresponding to the end of the helix.

For this Weddle's rule was used, viz.

$$\int_0^{6h} u_x\,dx = \frac{3}{10}h\,\{u_0+u_2+u_4+u_6+5(u_1+u_5)+6u_3\},$$

where 6^h is the length of the helix and u_x the magnetic intensity at any point.

The difference of magnetic potential at the ends for a certain current being known, the strength, N, of the helix (which is the ratio of this difference to the current, or the difference of magnetic potential which would be due to a unit current) is known, and is a *number*, because current and magnetic potential are of the same dimensions.

In the helix used, which was about 26·34 centims. in length and 13 centims. in diameter, we had

$$N = 10752.$$

The absolute value of the degrees of a tangent galvanometer was also determined by placing it under the dynamometer.

To determine the rotation of the plane of polarization, a Nicol's prism, set in a circle, was used, and the light was polarized by means of a prism invented by Professor Jellett, and described by him in vol. xxv. of the Transactions of the Royal Irish Academy.

It was constructed of Iceland spar, and its field of vision consisted of a circle divided by a line, the light of one half of which was polarized in a certain plane, and the light of the other half in a plane inclined at about 2° to that of the first. The intermediate position of the Nicol, when the whole field was equally dark, could be determined with some accuracy.

The water was contained in a tube with glass ends, of the same length as the helix, and placed with it. The polarized ray was sent through it,

and a current, whose intensity, C, was measured by the tangent galvanometer included in the circuit, was sent through the helix first in one direction and then in the other, and the plane of polarization observed. Half the difference of the readings was the rotation produced by the current.

If we call θ this rotation expressed in circular measure, and define Verdet's constant as the rotation which a unit current in a unit coil could produce in unit of length of distilled water, we have

$$\omega = \frac{\theta}{\text{N C}}.$$

The result of the series of experiments made was to obtain for ω the value

$$\omega = (10^{-7})\,4{\cdot}49 \text{ centimetre-gramme-seconds.}$$

Its dimensions obviously are the reciprocal of those of current, viz.

$$[\omega] = [\text{L}^{-\frac{1}{2}}\text{M}^{-\frac{1}{2}}\text{T}].$$

If we put our result in a slightly different form we may say that,

If plane polarized light passes through distilled water, and the magnetic potential of the water at any two points in the path of the ray differs by unity, then the plane of polarization will be rotated between those points $4\frac{1}{2}$ ten-millionths of a unit of circular measure.

Cavendish Laboratory, Cambridge,
April 30, 1875.

V. "On Rolling-Friction." By Professor Osborne Reynolds. Communicated by Dr. Balfour Stewart, F.R.S. Received May 24, 1875.

(Abstract.)

The motion of a roller or wheel on a surface is always attended with resistance. Coulomb made some experiments with wooden rollers on a wooden plane, from which he deduced two laws, viz. that the resistance is proportional to the weight of the roller, and inversely proportional to its diameter. These laws have since been found to apply to other substances, a different coefficient being used in each case. Beyond this, however, nothing appears hitherto to have been ascertained as regards the nature of this resistance to rolling. The source from which it springs does not appear to have been made the subject of investigation.

Some time ago it occurred to the author that it was probable that the deformation of the surface of the roller and of the plane, which must take place at the point of contact, would affect the distance which the roller would advance in turning through a certain angle*. The pressure of the roller on the plane causes a certain temporary indentation and

* The Engineer, 27th Nov., 1874.

lateral extension in the latter, so that in passing from one point to another the roller does in truth pass over a greater extent of surface than the distance between these points. A simple experiment was sufficient to verify the truth of this conclusion. An iron roller 18 inches in circumference was found to roll through something like $\frac{3}{4}$ inch less than a yard in two complete revolutions when rolling on a plate of india-rubber. The softness of the india-rubber suffered the roller to indent it considerably; and hence it might be expected that the effect would be much more apparent than when the roller was rolling on iron or any hard material. At the same time there is doubtless a certain amount of indentation in this latter case; and this will probably cause a similar alteration in the distance rolled through, although too small to allow its being measured.

This falling off from what may be called the geometrical distance, suggested an explanation of the resistance to rolling, namely, that the extension of the surface or surfaces at the point of contact causes the one surface to slide over the other; and this sliding is accomplished against friction. In this way we should expect to find the resistance to rolling greatest under those circumstances in which the sliding is greatest, *i. e.* where the indentation is greatest; and so far it is in accordance with Coulomb's laws. In the case of india-rubber, we find the slipping is very large; and hence we should expect the resistance to rolling to be large also; and accordingly we find it so, for it is more than ten times as great as when the roller is on an iron plane. This very great resistance which india-rubber causes to rolling appears not to have previously caught attention; and yet it is the natural explanation of the invariable failure which has attended the numerous endeavours which have been made to use this material for the tires of wheels.

This idea, that the resistance to rolling is due to the friction between the surfaces sliding at the point of contact, naturally leads to the conclusion that it must depend on the coefficient of friction between these surfaces, and that we might expect to diminish the resistance by using oil or any other means of reducing the coefficient of friction. This was the author's first impression. Experiments, however, showed that the effect of oiling the surface, although it did generally reduce the resistance, was very small; and sometimes it appeared to act in the reverse manner, and increase the resistance. This conclusion or surmise was therefore wrong; and the cause of the error was not far to seek. It consisted in having overlooked the fact that friction not only opposes the sliding of the one surface over the other, but also prevents it to a considerable extent, and thus modifies the deformation which would otherwise take place; so that any diminution in the coefficient of friction is attended with an increase in the extent of slipping, which tends to balance the advantage gained by the reduced coefficient.

The truth of this view derives independent support from a circumstance remotely connected with rolling-friction, of which it furnishes an

explanation. When the roller rests on a horizontal surface and is very slightly disturbed, it does not move off, but oscillates backwards and forwards. This happens on all kinds of elastic surfaces; on soft india-rubber the oscillations are both large and continue for some time. Now if the deformation in the surface of the rubber were complete, there would be no tendency to bring the roller back; but since, owing to friction, the india-rubber, under the advancing side of the roller, is prevented from extending while that under the other side is prevented from contracting, there will exist a state of constraint from which the surface is endeavouring to free itself by forcing the roller back.

Besides the relative softness of the materials, the curvature of the roller will affect the lateral extension both of the roller and the plane at the point of contact, so that if the roller and the plane were of the same material there would still be slipping. This would not be the case, however, between two wheels of the same diameter and material rolling in contact.

Such is a short sketch of the subject of the paper, a considerable part of which is devoted to the examination and illustration of the exact manner in which the deformation at the point of contact occurs, and the influence of friction upon it. The latter part of the paper contains an account of numerous experiments, and their results, which were undertaken as part of this investigation.

The first series of experiments relate to the resistance which an iron roller experiences on surfaces of different hardness. Cast iron, glass, brass, boxwood, and india-rubber were tried. Extreme care was taken to make the roller and the surfaces true; and this was so far successful that on cast iron the roller would roll in either direction when the surface had an inclination of one in five thousand, or, roughly, a foot in a mile. Comparing the different surfaces, we see that the resistance increases with the softness, although apparently not in the simple proportion; on boxwood the resistance is nearly double as great as on the harder surfaces, and on india-rubber from six to ten times as great.

The second series of experiments were to ascertain the actual extent of slipping on india-rubber, both with a cast-iron roller and also with an india-rubber tire glued on to the roller, and rolled on hard surfaces and on plates of india-rubber of different thicknesses.

These experiments bear out the arguments expressed in the first part of the paper; in fact the arguments were based on the experiments. There is no intention to imply that the whole of the resistance to rolling is in all cases due to the causes already mentioned. Under ordinary circumstances the irregularities of the surfaces and the crushing of the material beneath the roller are the chief causes. And, besides these, two other causes are discussed in the paper as having been brought to light by the experiment, viz. the communication of heat between the compressed material and that which surrounds it, which prevents the material im-

mediately expanding to the same volume as it previously occupied, and the viscosity of the material, which also renders it slow to expand. Both these causes are, however, rather connected with the effect of the speed of the roller on the resistance than with the residual resistance, which, so far as the surfaces are perfectly true and perfectly hard, appears to be due to the friction which accompanies the deformation, and is hence called *rolling-friction.*

No attempt has yet been made to investigate the laws of rolling-friction, although the author hopes to continue the investigation in this direction as soon as he has obtained the necessary apparatus.

At the end of the paper attention is called to certain phenomena connected with railway-wheels, which it is thought now, for the first time, receive an explanation. Thus the surprising superiority of steel rails over iron in point of durability is explained as being due as much to the fact that their hardness prevents the wearing-action, *i. e.* the slipping, as that it enables them better to withstand the wear. Also the slipping beneath the wheel explains the wear of the rails in places where brakes are not applied; and the severe lateral extension beneath the wheel is thought to explain the scaling of wrought-iron rails.

VI. "On Multiple Contact of Surfaces." By WILLIAM SPOTTISWOODE, M.A., Treas. R.S. Received May 24, 1875.

(Abstract.)

In a paper "On the Contact of Quadrics with other Surfaces," published in the Proceedings of the London Mathematical Society (May 14, 1874, p. 70), I have shown that it is not in general possible to draw a quadric surface V so as to touch a given surface U in more than two points, but that a condition must be fulfilled for every additional point. The equations expressing these conditions, being interpreted in one way, show that two points being taken arbitrarily, the third point of contact, if such there be, must lie on a curve, the equation whereof is there given. The same formulæ, interpreted in another way, serve to determine the conditions which the coefficients of the surface V must fulfil in order that the contact may be possible for three or more points taken arbitrarily upon it; and, in particular, the degrees of these conditions give the number of surfaces of different kinds which satisfy the problem.

In another paper, "Sur les Surfaces Osculatrices" (Comptes Rendus, 6 Juillet, 1874, p. 24), the corresponding conditions for the osculation of a quadric with a given surface are discussed.

In the present paper I have regarded the question in a more general way; and having shown how the formulæ for higher degrees of contact

are obtained, I have developed more in detail some special cases of interest.

For the convenience of the reader, I have in § 1 briefly recapitulated the principal parts of the two papers above quoted. In § 2 I have given, at all events, a first sketch of a general theory of multiple contact with quadrics; in § 3 the particular cases of three-, four-, five-, and six-pointic contact are discussed; and in § 4 some conditions for the existence of points of four-, five-, six-pointic single (*i. e.* not multiple) contact are established.

Thus far the investigation concerns the contact of quadrics only with other surfaces. The concluding part of the paper is concerned with the corresponding problem for cubics, in which case conditions of possibility do not arise for simple or two-pointic contact, but are first met with for three-pointic contact. The conditions in question, with some of their consequences, are here given; and their complexity will perhaps be sufficient justification for not pursuing the subject further in this direction.

VII. "On the Theory of the Solution of a System of Simultaneous Non-linear Partial Differential Equations of the First Order." By E. J. Nanson. Communicated by Prof. Cayley, F.R.S. Received June 5, 1875.

(Abstract.)

Given an equation of the form

$$z=\phi(x_1, x_2, \ldots . x_{n+m}, a_1, a_2, \ldots a_n),$$

we obtain by differentiation with respect to each of the $n+m$ independent variables $x_1, x_2, \ldots . x_{n+m}$, and elimination of the n arbitrary constants $a_1, a_2, \ldots a_n$, a system of $m+1$ non-linear partial differential equations of the first order. Of this system the given equation may be said to be a "complete primitive."

Conversely, given a system of non-linear partial differential equations of the first order, it is proposed to determine the conditions which must be satisfied in order that the system may admit of a complete primitive, and also to examine what kind of solution, if any, exists when the conditions above referred to are not satisfied.

The late Professor Boole has given an elegant method of treating a system of linear partial differential equations of the first order; but the present memoir relates to a more general system, which appears not to have been hitherto considered, viz. to a non-linear system of partial differential equations. This is here discussed in the two cases—first, when the dependent variable z is not explicitly involved in the proposed system; and, secondly, when z is explicitly involved in the system, the solution of this last case being made to depend upon that of the first-mentioned one.

VIII. "Reduction of Anemograms taken at Armagh Observatory in the Years 1857 to 1863." By T. R. Robinson, D.D., F.R.S., &c. Received June 11, 1875.

The instrument with which these observations were made is described in the Transactions of the Royal Irish Academy, vol. xxii., and a continuous series of its records exist from 1845 to 1870. With the limited resources of this Observatory it was not in my power to reduce them; but it seemed to some distinguished members of the Royal Society desirable to ascertain whether such observations are competent to develop any laws amid the seeming lawlessness of the winds, and they obtained for me a grant from the Government Fund to discuss the anemograms of these seven years. Unfortunately the work has been long delayed by various accidents.

Of the causes of wind some are undoubtedly periodical; and though they are masked by others of greater magnitude, which, in the present state of our knowledge, seem quite lawless, yet these will disappear from the mean of a sufficient number of observations and leave as residual the first. Of the periodical causes the unequal distribution of heat is the most important; and this, depending on the place of the sun, is evidently a function of the time.

The immediate data of the anemograms, the velocity and direction of the wind, though not the most convenient for combining in great numbers, yet are those which interest most directly the general inquirer; and I have presented them in a Table, which shows for each month of the seven years the mean velocity of the wind in each octant, the number of hours during which it has blown, the maximum in each month, the number of hours above 25 miles, and the number during which the record =0. The most striking fact shown by this Table is its extreme irregularity, not merely from octant to octant or month to month, but from year to year. Both velocity and hours are a maximum in the octant S.SW., a minimum in N.NE., their products being as 6 : 1. As to monthly variations, the amount of wind is a maximum in January, decreasing to July in the ratio of $2\frac{1}{4}:1$, and thence increasing to the end of the year, with an exception in the case of March, which is greater than February as 1·13 : 1. This, however, does not establish the common idea of equinoctial gales; for the hours above 25 miles are fewer in March than in February, and there is no excess in September above October. There is also no clear indication of any influence of the solar spots; but for detecting this several decennial periods will be necessary. The annual variations are equally notable. The maximum velocity ranges from 71 in 1861 to 19 in 1860. If the mean velocity for each month be taken without reference to direction, it is 13·51 for January, 4·24 for June, and that for the whole year is 9·73. A mode of discussion which seems more likely to give definite laws is to resolve each velocity into a southern

component S and a western W, to deduce from these interpolation formulæ involving periodic functions of the time for periods of one or more years, and from the changes of these functions in successive periods to derive some general laws. At first sight this might seem impracticable, from the excessive discordance of the values for the same in different years. Thus in the first term of the set, January 1^d 0^h, the extreme difference in the seven years is, for W 20·89, for S 25·35. Evidently single hours were out of the question, and even the mean for the seven years, as was evident from examining their probable errors. However, I meaned each hour for the seven years, then combined these in periods of ten days, but ultimately took their mean for the entire month. These monthly means are given in Table III., from which it appears, first, that all the ultimate values of W and S are positive. This arises from the preponderance of positive over negative values; but the latter occur so frequently that they evidently belong to the wind system; and I was at first disposed to mean and develop them separately. I tried it for January and June, but saw that in the present state of our knowledge it would be useless. In January the negative values are 0·27 of the whole, in June 0·374; and they are found in the septennial means of almost every hour, but so irregularly distributed that it would be almost impossible to develop them in terms of the time. Even were this done, we could not combine in any particular instance the negative and positive results unless we knew the causes which occasionally mix the polar and equatorial currents. I therefore took the entire means as alone available. Secondly, that, as I anticipated, notwithstanding the discordance of the individual observations, the means of from 196 to 217 present a notable agreement, and the differences which they exhibit are subject to law. If we examine the vertical columns (which give the hours of each month), we find in each a principal maximum and minimum and one or more lesser ones. The epochs of these vary with the season. For W in winter the chief maximum is from noon to 3 P.M., in summer from 9 A.M. to noon; for S it varies less, being a little before noon. The principal minimum occurs from 6 P.M. to 10 P.M., both for W and S.

The extreme diurnal ranges are greatest in March, 2·14 and 2·40; least in November, 0·74 and 0·79.

Examining the horizontal columns, which give the monthly variation, the existence of law is still more evident. W has a maximum in January, a minimum in February; its greatest maximum in March, its least minimum in April; a smaller maximum in August, and a smaller minimum in November. The variations are greater here than in the horary columns.

The law for S is simpler: it has one maximum in December and one minimum in July; its range, too, is something greater.

The mean W for the whole seven years = 2·4805 miles, the mean S = 3·8398 miles, which give the mean V = 4·5713, mean D = 32° 54′ 44″, and the actual translation of air 39648 miles.

A Table whose data belong to dates separated by considerable intervals will not give the components generally without interpolation. The formula universally adopted for this when the quantities concerned are periodic functions of the time-angle is that given by Bessel—

$$\underset{\theta}{\mathrm{U}}=\mathrm{K}+\mathrm{A}\cos\theta+\mathrm{B}\cos 2\theta\ \&\mathrm{c}.,+\mathrm{O}\sin\theta+\mathrm{P}\sin 2\theta\ \&\mathrm{c}.,$$

or its secondary equivalent—

$$\underset{\theta}{\mathrm{U}}=\underset{0}{\mathrm{K}}+\underset{'}{\mathrm{K}}\sin(\kappa_{'}+\theta)+\underset{''}{\mathrm{K}}\sin(\kappa_{''}+2\theta)+\ \&\mathrm{c}.,$$

where θ is the hour-angle from midnight. But as the monthly variations must also be represented, the coefficients of the first equation must be developed in terms of ϕ (the time-angle from the beginning of the year), and the expression of each of them multiplied by the corresponding cosine or sine of θ. Bessel's computation of the coefficients may be much shortened where, as in the cases before us, the circle is divided into $2n$ equal parts (n being an integer), and the first term of the series $=0$ or $\frac{\pi}{2n}$; for, in consequence of the numerical equality of the cosine and sine of θ, $180+\theta$, $180-\theta$, and $360-\theta$, it is only necessary to compute for the first quadrant. For the horary sets this labour might be shortened by combining them in groups of 3; and the formula for this is given, but it is not quite as exact as the ordinary one, which is also given. The horary constants for W and S, computed by this last, are given in Tables V. and VI. for each month to the fourth order, and an estimate of their precision.

These constants are then developed in month-time, for which the formula is given. This, however, requires a correction; it supposes each u from which it is deduced to belong to a series of ϕ in arithmetical progression. This is not the case: first, the mean of each month does not represent the u belonging to the middle of that month; secondly, the angular distances of the middle of each month from the beginning of the year are not in arithmetical progression. These are both corrected by multiplying the constants by certain factors. The secondary constants so corrected are given in Table VIII. to the 6th order.

As an example of the mode of trying what effect any periodical agent may have on the coordinates, the sun's altitude at Armagh is considered. It is developed in terms of θ, and may probably account for 0·27 of the variation of W and 0·53 of that of S.

The paper concludes with an attempt to show from these observations the existence of an aerial tide-current, which, according to Laplace, is at its maximum 0·195 mile per hour. There was little hope of detecting so small a quantity; but the attempt would at least show how far the mean of a large number of observations may approach the truth. When the moon is east of the meridian its attraction increases W, when west

lessens it; and without attempting to allow for elongation from the sun or declination, I merely compared the Ws at the lunar hours 21 and 3, 9 and 15. I only took the first six months, which seemed sufficient. The result rather surprised me; 2418 observations give for the current 0·0906, which, allowing for the omissions above mentioned and for friction at the earth's surface, must be very near the truth. Among the observations are two above 40 and three above 30; and it seemed worth trying what would be the effect of omitting these and all above four times the probable error of one. In this case for W–W′ it was all above 15. The result is that 2360 observations give 0·0559, showing how little even considerable discordances affect a mean under such circumstances, and also perhaps that even such discordances should not be rejected.

IX. "Preliminary Notice of further Researches on the Physical Properties of Matter in the Liquid and Gaseous States under varied conditions of Pressure and Temperature." By Dr. Andrews, F.R.S., Vice-President of Queen's College, Belfast. Received June 17, 1875.

The investigation to which this note refers has occupied me, with little intermission, since my former communication in 1869 to the Society, "On the Continuity of the Liquid and Gaseous States of Matter." It was undertaken chiefly to ascertain the modifications which the three great laws discovered respectively by Boyle, Gay-Lussac, and Dalton undergo when matter in the gaseous state is placed under physical conditions differing greatly from any hitherto within the reach of observation. It embraces a large number of experiments of precision, performed at different temperatures and at pressures ranging from twelve to nearly three hundred atmospheres. The apparatus employed is, in all its essential parts, similar to that described in the paper referred to; and so perfectly did it act that the readings of the cathetometer, at the highest pressures and temperatures employed, were made with the same ease and accuracy as if the object of the experiment had been merely to determine the tension of aqueous vapour in a barometer-tube. In using it the chief improvement I have made is in the method of ascertaining the original volumes of the gases before compression, which can now be known with much less labour and greater accuracy than by the method I formerly described. The lower ends of the glass tubes containing the gases dip into small mercurial reservoirs formed of thin glass tubes, which rest on ledges within the apparatus. This arrangement has prevented many failures in screwing up the apparatus, and has given more precision to the measurements. A great improvement has also been made in the method of preparing the leather-washers used in the packing for the fine screws, by means of which the pressure is obtained. It consists in saturating the leather with grease by heating it *in vacuo*

under melted lard. In this way the air enclosed within the pores of the leather is removed without the use of water, and a packing is obtained so perfect that it appears, as far as my experience goes, never to fail, provided it is used in a vessel filled with water. It is remarkable, however, that the same packing, when an apparatus specially constructed for the purpose of forged iron was filled with mercury, always yielded, even at a pressure of 40 atmospheres, in the course of a few days.

It is with regret that I am still obliged to give the pressures in atmospheres as indicated by an air- or hydrogen-manometer, without attempting for the present to apply the corrections required to reduce them to true pressures. The only satisfactory method of obtaining these corrections would be to compare the indications of the manometer with those of a column of mercury of the requisite length; and this method, as is known, was employed by Arago and Dulong, and afterwards in his classical researches by Regnault, for pressures reaching nearly to 30 atmospheres. For this moderate pressure a column of mercury about 23 metres, or 75 feet, in length had to be employed. For pressures corresponding to 500 atmospheres, at which I have no difficulty in working with my apparatus, a mercurial column of the enormous height of 380 metres, or 1250 feet, would be required. Although the mechanical difficulties in the construction of a long tube for this purpose are perhaps not insuperable, it could only be mounted in front of some rare mountain escarpment, where it would be practically impossible to conduct a long series of delicate experiments. About three years ago I had the honour of submitting to the Council of the Society a proposal for constructing an apparatus which would have enabled any pressure to be measured by the successive additions of the pressure of a column of mercury of a fixed length; and working drawings of the apparatus were prepared by Mr. J. Cumine, whose services I am glad to have again this opportunity of acknowledging. An unexpected difficulty, however, arose in consequence of the packing of the screws (as I have already stated) not holding when the leather was in contact with mercury instead of water, and the apparatus was not constructed. For two years the problem appeared, if not theoretically, to be practically impossible of solution; but I am glad now to be able to announce to the Society that another method, simpler in principle and free from the objections to which I have referred, has lately suggested itself to me, by means of which it will, I fully expect, be possible to determine the rate of compressibility of hydrogen or other gas by direct reference to the weight of a liquid column, or rather of a number of liquid columns, up to pressures of 500 or even 1000 atmospheres. For the present it must be understood that, in stating the following results, the pressures in atmospheres are deduced from the apparent compressibility, in some cases of air, in others of hydrogen gas, contained in capillary glass tubes.

In this notice I will only refer to the results of experiments upon carbonic acid gas when alone or when mixed with nitrogen. It is with

carbonic acid, indeed, that I have hitherto chiefly worked, as it is singularly well adapted for experiment; and the properties it exhibits will doubtless, in their main features, be found to represent those of other gaseous bodies at corresponding temperatures below and above their critical points.

Liquefaction of Carbonic Acid Gas.—The following results have been obtained from a number of very careful experiments, and give, it is believed, the pressures, as measured by an air-manometer, at which carbonic acid liquefies for the temperatures stated:—

Temperatures in Centigrade degrees.		Pressure in atmospheres.
0	....	35·04
5·45	....	40·44
11·45	....	47·04
16·92	....	53·77
22·22	....	61·13
25·39	....	65·78
28·30	....	70·39

I have been gratified to find that the two results (for 13°·09 and 21°·46) recorded in my former paper are in close agreement with these later experiments. On the other hand, the pressures I have found are lower than those given by Regnault as the result of his elaborate investigation (Mémoires de l'Académie des Sciences, vol. xxvi. p. 618). The method employed by that distinguished physicist was not, however, fitted to give accurately the pressures at which carbonic acid gas liquefies. It gave, indeed, the pressures exercised by the liquid when contained in large quantity in a Thilorier's reservoir; but these pressures are always considerably in excess of the true pressures in consequence of the unavoidable presence of a small quantity of compressed air, although the greatest precautions may have been taken in filling the apparatus. Even $\frac{1}{500}$ part of air will exercise a serious disturbing influence when the reservoir contains a notable quantity of liquid.

Law of Boyle.—The large deviations in the case of carbonic acid at high pressures from this law appeared distinctly from several of the results given in my former paper. I have now finished a long series of experiments on its compressibility at the respective temperatures of 6°·7, 63°·7, and 100° Centigrade. The two latter temperatures were obtained by passing the vapours of pyroxylic spirit (methyl alcohol) and of water into the rectangular case with plate-glass sides in which the tube containing the carbonic acid is placed. The temperature of the vapour of the pyroxylic spirit was observed by an accurate thermometer, whose indications were corrected for the unequal expansion of the mercury; while that of the vapour of water was deduced from the pressure as given by the height of the barometer and a water-gauge attached to the apparatus. At the lower temperature (6°·7) the range of pressure which could be applied was limited by the occurrence of

liquefaction; but at the higher temperatures, which were considerably above the critical point of carbonic acid, there was no limit of this kind, and the pressures were carried as far as 223 atmospheres. I have only given a few of the results; but they will be sufficient to show the general effects of the pressure. In the following Tables p designates the pressure in atmospheres as given by the air-manometer, t' the temperature of the carbonic acid, ϵ the ratio of the volume of the carbonic acid under one atmosphere and at the temperature t' to its volume under the pressure p' and at the same temperature, and θ the volume to which one volume of carbonic acid gas measured at 0° and 760 millimetres is reduced at the pressure p and temperature t'.

Carbonic Acid at 6°·7.

p.		t'.		ϵ.		θ.
at.						
13·22	....	6°·90	....	$\frac{1}{14·36}$	....	0·07143
20·10	....	6·79	....	$\frac{1}{23·01}$	....	0·04456
24·81	....	6·73	....	$\frac{1}{29·60}$	....	0·03462
31·06	....	6·62	....	$\frac{1}{39·57}$	....	0·02589
40·11	....	6·59	....	$\frac{1}{58·40}$	....	0·01754

Carbonic Acid at 63°·7.

p.		t'.		ϵ.		θ.
at.						
16·96	....	63°·97	....	$\frac{1}{17·85}$	....	0·06931
54·33	....	63·57	....	$\frac{1}{66·06}$	....	0·01871
106·88	....	63·75	....	$\frac{1}{185·9}$	....	0·00665
145·54	....	63·70	....	$\frac{1}{327·3}$	....	0·00378
222·92	....	63·82	....	$\frac{1}{446·9}$	....	0·00277

Carbonic Acid at 100°.

p.		t'.		ϵ.		θ.
at.						
16·80	....	100°·38	....	$\frac{1}{17·33}$	....	0·07914
53·81	....	100·33	....	$\frac{1}{60·22}$	....	0·02278
105·69	....	100·37	....	$\frac{1}{137·1}$	....	0·01001
145·44	....	99·46	....	$\frac{1}{218·9}$	....	0·00625
223·57	....	99·44	....	$\frac{1}{380·9}$	....	0·00359

These results fully confirm the conclusions which I formerly deduced from the behaviour of carbonic acid at 48°, viz. that while the curve representing its volume under different pressures approximates more nearly to that of a perfect gas as the temperature is higher, the contraction is nevertheless greater than it would be if the law of Boyle held good, at least for any temperature at which experiments have yet been made. From the foregoing experiments it appears that at 63°·7 carbonic acid gas, under a pressure of 223 atmospheres, is reduced to $\frac{1}{447}$ of its volume under one atmosphere, or to less than one half the volume it ought to occupy if it were a perfect gas and contracted in conformity with Boyle's law. Even at 100° the contraction under the same pressure amounts to $\frac{1}{381}$ part of the whole. From these observations we may infer by analogy that the critical points of the greater number of the gases not hitherto liquefied are probably far below the lowest temperatures hitherto attained, and that they are not likely to be seen, either as liquids or solids, till much lower temperatures even than those produced by liquid nitrous oxide are reached.

Law of Gay-Lussac.—That the law of Gay-Lussac in the case of the so-called permanent gases, or in general terms of gases greatly above their critical points, holds good at least at ordinary pressures, within the limits of experimental error, is highly probable from the experiments of Regnault; but the results I have obtained with carbonic acid will show that this law, like that of Boyle, is true only in certain limiting conditions of gaseous matter, and that it wholly fails in others. It will be shown that not only does the coefficient of expansion change rapidly with the pressure, but that, *the pressure or volume remaining constant, the coefficient changes with the temperature.* The latter result was first obtained from a set of preliminary experiments, in which the expansion of carbonic acid under a pressure of 17 atmospheres was observed at 4°, 20°, and 54°; and it has since been fully confirmed by a large number of experiments made at different pressures and well-defined temperatures. These experiments were conducted by the two methods commonly known as the method of constant pressure and the method of constant volume. The two methods, except in the limiting conditions, do not give the same values for the coefficient of expansion; but they agree in this respect, that at high pressures the value of that coefficient changes with the temperature. While I have confined this statement to the actual results of experiment, I have no doubt that future observations will discover, in the case, at least, of such gases as carbonic acid, a similar but smaller change in the value of the coefficient for heat at low pressures. The numerous experiments I have made on this subject will shortly be communicated in detail to the Society; and for the present I will only give the following results:—

Expansion of Heat of Carbonic Acid Gas under high pressures.

Pressure.	Vol. CO_2 at 0° & 760 millims.=1.	Vol. CO_2 at 6°·05 & 22·26 at.=1.	Temperature.	
at.			°	
22·26	0·03934	1·0000	6·05	(A)
22·26	0·05183	1·3175	63·79	
22·26	0·05909	1·5020	100·10	

Pressure.	Vol. CO_2 at 0° & 760 millims.=1.	Vol. CO_2 at 6°·62 & 31·06 at.=1.	Temperature.	
at.			°	
31·06	0·02589	1·0000	6·62	(B)
31·06	0·03600	1·3905	63·83	
31·06	0·04160	1·6068	100·64	

Pressure.	Vol. CO_2 at 0° & 760 millims.=1.	Vol. CO_2 at 6°·01 & 40·06 at.=1.	Temperature.	
at.			°	
40·06	0·01744	1·0000	6·01	(C)
40·06	0·02697	1·5464	63·64	
40·06	0·03161	1·8123	100·60	

Taking as unit 1 vol. of carbonic acid at 6°·05 and 22·26 atmospheres, we obtain from series A the following values for the coefficient of heat for different ranges of temperature :—

α=0·005499 from 6°·05 to 63°·79.
α=0·005081 from 63°·79 to 100°·1.

From series B, with the corresponding unit volume at 6°·62 and 31·06 atmospheres, we find :—

α=0·006826 from 6°·62 to 63°·83.
α=0·005876 from 63°·83 to 100°·64.

And in like manner from series C with the unit volume at 6°·01 and 40·06 atmospheres :—

α=0·009481 from 6°·01 to 63°·64.
α=0·007194 from 63°·64 to 100°·60.

The coefficient of carbonic acid under one atmosphere referred to a unit volume at 6° is

α=0·003629.

From these experiments it appears that the coefficient of expansion increases rapidly with the pressure. Between the temperatures of 6° and 64° it is once and a half as great under 22 atmospheres, and more than two and a half times as great under 40 atmospheres, as at the pressure of 1 atmosphere. Still more important is the change in the value of the coefficient at different parts of the thermometric scale, the pressure remaining the same. An inspection of the figures will also show that this change of value at different temperatures increases with the pressure.

Another interesting question, and one of great importance in reference to the laws of molecular action, is the relation between the elastic forces of a gas at different temperatures while the volume remains constant. The experiments which I have made in this part of the inquiry are only preliminary, and were performed not with pure carbonic acid, but with a mixture of about 11 volumes of carbonic acid and 1 volume of air. It will be convenient, for the sake of comparison, to calculate, as is usually done, the values of α from these experiments; but it must be remembered that α here represents no longer a coefficient of volume, but a coefficient of elastic force.

Elastic force of a mixture of 11 vol. CO_2 and 1 vol. air heated under a constant volume to different temperatures.

Vol. CO_2.		Temperature.		Elastic Force.	
		°		at.	
366·1	....	13·70	....	22·90	(A)
366·2	....	40·63	....	25·74	
366·2	....	99·73	....	31·65	
256·8	....	13·70	....	31·18	(B)
256·8	....	40·66	....	35·44	
256·8	....	99·75	....	44·29	

From series A we deduce for a unit at 13°·70 and 22·90 atmospheres:—

$$\alpha=0{\cdot}004604 \text{ from } 13^\circ{\cdot}70 \text{ to } 40^\circ{\cdot}63.$$
$$\alpha=0{\cdot}004367 \text{ from } 40^\circ{\cdot}63 \text{ to } 99^\circ{\cdot}73.$$

And from series B:—

$$\alpha=0{\cdot}005067 \text{ from } 13^\circ{\cdot}70 \text{ to } 40^\circ{\cdot}66.$$
$$\alpha=0{\cdot}004804 \text{ from } 40^\circ{\cdot}66 \text{ to } 99^\circ{\cdot}75.$$

The coefficient at 13°·70 and 1 atmosphere is

$$\alpha=0{\cdot}003513.$$

It is clear that the changes in the values of α, calculated from the elastic forces under a constant volume, are in the same direction as those already deduced from the expansion of the gas under a constant pressure. The value of α increases with the pressure, and it is greater at lower than at higher temperatures. But a remarkable relation exists between the coefficients in the present case which does not exist between the coefficients obtained from the expansion of the gas. The values of α, deduced for the same range of temperature from the elastic forces at different pressures, are directly proportional to one another. We have, in short,

$$\frac{0{\cdot}004367}{0{\cdot}004604}=0{\cdot}9485, \qquad \frac{0{\cdot}04804}{0{\cdot}05067}=0{\cdot}9481.$$

How far this relation will be found to exist under other conditions of temperature and pressure will appear when experiments now in progress are brought to a conclusion.

Law of Dalton.—This law, as originally enunciated by its author, is, that the particles of one gas possess no repulsive or attractive power with regard to the particles of another. "Oxygen gas," he states, "azotic gas, hydrogenous gas, carbonic acid gas, aqueous vapour, and probably several other elastic fluids may exist in company under any pressure and at any temperature without any regard to their specific gravities, and without any pressure upon one another." The experiments which I have made on mixtures of carbonic acid and nitrogen have occupied a larger portion of time than all I have yet referred to. They have been carried to the great pressure of 283·9 atmospheres, as measured in glass tubes by a hydrogen manometer, at which pressure a mixture of 3 volumes carbonic acid and 4 volumes nitrogen was reduced at 7°·6 to $\frac{1}{378}$ of its volume without liquefaction of the carbonic acid. As this note has already extended to an unusual length, I will not now attempt to give an analysis of these experiments, but shall briefly state their general results. The most important of these results is *the lowering of the critical point by admixture with a non-condensable gas.* Thus in the mixture mentioned above of carbonic acid and nitrogen, no liquid was formed at any pressure till the temperature was reduced below $-20°$ C. Even the addition of only $\frac{1}{10}$ of its volume of air or nitrogen to carbonic acid gas will lower the critical point several degrees. Finally, these experiments leave no doubt that the law of Dalton entirely fails under high pressures, where one of the gases is at a temperature not greatly above its critical point. The anomalies observed in the tension of the vapour of water when alone and when mixed with air find their real explanation in the fact that the law of Dalton is only approximately true in the case of mixtures of air and aqueous vapour at the ordinary pressure and temperature of the atmosphere, and do not depend, as has been alleged, on any disturbing influence produced by a hygroscopic action of the sides of the containing vessel. The law of Dalton, in short, like the laws of Boyle and Gay-Lussac, only holds good in the case of gaseous bodies which are at feeble pressures and at temperatures greatly above their critical points. Under other conditions these laws are interfered with; and in certain conditions (such as some of those described in this note) the interfering causes become so powerful as practically to efface them.

X. "On the Power of the Eye and the Microscope to see Parallel Lines." By J. A. Broun, F.R.S. Received June 16, 1875.

Since Nobert's test-lines have been employed for the purpose of determining the comparative powers of microscopes, several curious speculations have been made and conclusions arrived at by different well-known microscopists as to the ultimate capability of that instrument and even as to the ultimate atoms of matter. The lines in Nobert's test-bands were believed to approach to each other in a regularly diminishing series of distances, such that when the intervals left by the graving-point were about $\frac{1}{80,000}$ of an English inch wide, no microscope, however high the theoretical power, could show the separation of the lines.

The Jury of the Great Exhibition of 1851, in the Report (p. 268), said that to see the lines in the first and second bands of a Nobert's test-plate of 10 bands a power of 100 was sufficient, whereas to distinguish those of the tenth band a magnifying-power of 2000 was required. Dr. Lardner (in his Museum of Science and Art) considered this assertion erroneous, and stated that if a power of 100 could show the lines when there were 11,000 to the inch (as in the first band), a power of 450 should show the lines when there were 50,000 to the inch (as in the tenth band). As there can be no doubt of the accuracy of the Jury's statement, there has been considerable difficulty in ascertaining the cause of the great difference betwixt fact and theory; and the most absurd hypotheses have been suggested for this end. This and the speculations of different microscopists induced me several years ago to begin the examination of this question.

The first thing requiring to be clearly understood was, what the microscopes were employed to examine, what Nobert's test-bands really were. Some microscopists stated the number of lines to the inch when they could not be seen to be about 80,000; whereas Dr. Carpenter (in his work on the Microscope and its Revelations) said "it was a matter of faith whether lines existed at a narrower interval than $\frac{1}{84,000}$ of an inch." In this case if the lines were as wide as the intervals, the number to the inch would be 42,000. It was then essential to know what were the widths of the lines and intervals in the different bands, whether as the intervals would appear equally light, the lines were equally dark; and, finally, with reference to the bands in which the lines were so close that they could not be seen, whether there really were lines at all, or lines which really ought to be seen were the microscope practically equal to its theoretical power—whether, in fact, the failure was not rather in the engraving-machine than in the microscope.

It was only in 1869 that I was able, through the kindness of Mr. Eulenstein, of Dresden (previously of Cannstadt), to examine the magni-

fied photographs of Nobert's test-bands by Dr. E. Carter, Surgeon of the U.S. Army. The results of this examination will be given at the end of this note.

I was induced meanwhile to examine the power of the eye, in order to compare it with the power of the microscope, and to determine what a microscope of given power should be able to show. The following observations had been made early in 1869, before I was acquainted with the observations of Dr. Jurin and of Tobias Mayer, to which I shall allude.

The first question which presented itself was as to the power of the eye to see single lines under the ordinary illumination of a northern sky.

1*st observation.*—A black line 0·042 inch wide, 1·75 inch long, drawn with common writing-ink on white paper, and a white line of the same width and length between two black lines, each 0·20 inch wide, were seen equally well within a room lighted by a window to N.W. at a distance of 30 feet, the angle subtended by the width of the lines being 24″ nearly.

2*nd observation.*—A dark-brown hair, 0·0026 inch wide, 2·5 inches long, was fixed by dots of transparent gum-arabic to the window-pane, and was seen against the N.W. sky by a young eye at 36 feet (I could not see it myself at a greater distance than 30 feet): the diameter of the hair subtended an angle of 1″·24 at the eye. The same eye examined fine lines divided on glass at a distance of 6 inches, and, other things equal, should have been able to see a line $\frac{1}{27,700}$ inch wide at that distance. [June 5, 1875. I find that a young eye can see lines on glass $\frac{1}{10,000}$ inch wide, $\frac{1}{28}$ inch long, angle 3″·5 nearly.]

Dr. Jurin could see a silver wire $\frac{1}{485}$ inch diameter placed on white paper when the diameter subtended an angle of 3″·5, and a silk fibre one fourth the diameter of the wire when it subtended an angle of 3″·35*.

3*rd observation.*—Whether the length of the line affects its visibility. The hair just observed was cut into pieces of different lengths and fixed, as before, to the window-pane; they could be seen at the following distances:—

Length of hair.	Distance seen.	Angle subtended by	
		Diameter.	Length.
in.	feet.		
0·90	37	1″·21	413″
0·25	32	1·39	134
0·133	22	2·03	104
0·020	10	4·46	86

* See Jurin's essay on distinct and indistinct vision in Smith's 'Complete System of Opticks,' 1738. I am acquainted with Jurin's observations from the Rev. Father Pezenas's translation of Smith's work, 'Cours complet d'Optique,' 1767, p. 282.

The hair 0·9 inch long was one foot further off than that 2·5 inch long on the day preceding (2nd observation). The difference was due, partly at least, to the different light of the sky.

4th observation.—The previous observation shows that the line is seen at a greater distance as the length increases till a limiting angle is attained, after which increase of length has no effect on the visibility. The following observations were made to determine approximately the law which relates visibility to length.

Lines of different lengths, 0·045 inch wide, were drawn on different slips of white paper (5·8 by 4·5 inches); the papers were pinned successively to a plank placed vertically in the shade (out of doors) with a clear sky (April, 6 P.M.); the mean distance of disappearance on retiring and of appearance on approaching the lines was taken.

Length.	Distance.	Angle subtended by			$\alpha\sqrt[3]{\beta}$.
		Length β.	Width α. Observed.	Width α. Calculated.	
in.	feet.	$''$	$''$	$''$	
0·045	......	......	......	26·4	
0·125	53	41	14·6	14·5	50
0·245	68	62	11·4	11·2	45
0·470	84	96	9·2	9·2	42
0·970	100	167	7·7	7·7	42
1·800	114	272	6·8	6·8	44
3·400	129	453	6·0	6·0	46

It will be seen that as the lengths of the lines increase in a geometrical progression (nearly), the distances increase in an arithmetical progression (nearly). It has been easy then to represent the observed angles (α) by the following equation:—

$$\alpha'' = \frac{14·5}{\log l - 1·10}, \quad \ldots \ldots \ldots \quad (1)^*$$

where α is expressed in seconds of arc, and l, the length of the line, is in units of 0·001 inch. The calculated values agree very nearly with those observed. The angle for a square of 0·045 inch calculated is 26″·4, which is very near to the value observed on a previous occasion. α (or $\tan\alpha$) becomes infinite for $l=12·6$ (0·0126 inch); but the formula does not hold for lines in which the length is less than the width. These belong to another case, that in which the lengths of the lines are constant and the width variable.

Having examined the power of the eye to see single lines, I now sought how this power would be affected when more lines than one were placed parallel to each other, and with intervals equal to their widths.

* This equation may be put under the following form, where D, the distance of the observer, and l are in units of one inch:—

$$D = 642 \log 79·4\, l.$$

5th observation.—Four dark-brown hairs having been arranged on paper at equal intervals by means of modeller's wax attached to the ends, and then fixed at the ends with gum-arabic, the paper was cut away between the ends, and the two slips of paper to which the ends were gummed were fixed to the window-pane: the diameter of the hairs was $\frac{1}{375}$ inch (0·00267) very nearly, the lengths were nearly 1 inch, and the intervals were very nearly the same as the diameter of the hair, the whole width being 0·019 inch. The hairs could be seen to be more than one at 28 inches distance, and they could be counted at 21 inches; the angles subtended by the intervals at these distances were 20″ and 26″·5 respectively.

It thus appears that at a distance greater than 28 inches the four hairs appeared as one, when each hair subtended an angle sixteen times greater than it could be observed at when seen alone (see the 3rd observation). This curious fact was pointed out by Dr. Jurin in the essay already cited. He found when two pins were placed near to each other on a window, that the interval between them could not be perceived when it subtended an angle of 40″, whereas a single pin could be seen at an angle of from 2 to 3 seconds. Mayer, who also made observations on parallel lines nearly twenty years after Jurin, does not seem to have remarked this fact*.

The 5th observation was repeated with four white hairs from a horse's tail; these were arranged at equal intervals, the mean diameter of the hair being 0·0105 and the mean interval 0·0110 inch. The hairs could be seen to be more than one 9 feet distant, when the angle subtended by each hair was 23″·1; and the hairs could be counted at 6 feet distant, when the angle was 30″·7. These angles are about one sixth greater than for the human hairs, the difference being probably due to the difference of light, and perhaps partly to the different length, which was not noted†.

6th observation.—A series of lines 0·7 inch long were drawn on separate slips of paper with different widths and intervals. The papers were fixed successively to the wall of a room lighted by a window to N.W., the light falling at an angle of about 45° on the paper. The following Table contains the results of the observations, first, when the intervals and lines were of equal width, and, second, when the intervals were 1, 2, 3, ... times the width of the lines.

* Mayer's observations are given in Pezenas's translation of Smith's 'Optics,' t. ii. p. 409. I am not acquainted with the original memoir. As already stated, Jurin's and Mayer's observations were known to me only after the above observations had been made.

† Dr. Jurin has given, as an example of the difficulty of counting parallel lines, the following series:—

and has shown the advantage of employing commas in such numbers as the following, 100000000000 and 100,000,000,000.

Lines and intervals equal.			Lines and intervals unequal.			
Mean width.	Distance.	Angle.	Width of Lines.	Width of Intervals.	Distance.	Angle.
in.	feet.	″	in.	in.	feet.	″
0·021	8·3	44	0·041	0·021	9·5	37·0
0·041	15·5	45	0·045	0·079	21·7	38·0
0·081	25·0	56	0·043	0·121	28·5	26·0
0·118	36·7	55	0·044	0·164	34·0	22·0
0·164	47·5	59	0·044	0·203	39·0	19·4
			0·045	0·241	40·5	19·0
			0·044	0·477	55·5	13·5

When the lines and intervals were equal, the angle increased from the smallest (0·021 inch) to the largest (0·164 inch); the greater angle for the middle width (0·081) is probably connected with some irregularity in the lines; the increase of the angle at the greater distance is probably chiefly due to the constancy of the length of the lines (see 8th observation).

When the lines and intervals are unequal, the angle subtended by the width of the line diminishes as the interval increases; the limiting angle would be that at which a *single* line 0·70 inch long and 0·044 inch wide would disappear.

7th observation.—It was now sought to determine in what degree the angle of visibility for parallel lines varied with the relative darkness of the lines, the intervals between the lines remaining equally bright. One drop of a weak writing-ink having been mixed with thirty drops of water, four lines, 0·70 inch long, 0·081 inch wide, were made (at intervals of 0·081 inch) with a camel-hair pencil; the first set (I_1) received only one coat, the second (I_2) two coats, and I_5 five coats. The slips of paper having been pinned successively to the wall of the room as before, the following are the results of the observations:—

Tint.	Distance. feet.	Angle. ″	Calculated. ″
I_1	15·5	90·0	91·0
I_2	19·5	71·5	69·7
I_3	21·5	64·7	63·1
I_4	23·0	60·5	60·0
I_5	23·6	59·0	58·4
⋮	⋮	⋮	⋮
I_n	25·0	55·7	56·0

The last (I_n) is the observation already given (6th observation) for the same lines made with a drawing-pen and as black as they could be made. It was found that one hundred coats of watered ink did not make lines so dark as those of I_n; and it is obvious that increase of darkness after the fifth coat (a very faint shade) made little difference in the visibility of the

lines. On the other hand, the tint may be made so faint as to be imperceptible at 6 inches (the distance for a young eye of faint or small objects). The preceding observations are represented nearly by the following formula:—

$$\alpha - 56 = \frac{19 \cdot 2}{1 \cdot 55^t - 1}, \qquad (2)$$

where α is the smallest angle (in seconds) at which the separation of the lines was visible, and t is the *tint* or number of the coats of watered ink. The calculated values are given in the Table.

When $t = \frac{1}{46}$ (of the first tint) the lines, according to the formula, should have been just visible at 8 inches from the eye, or a weaker shade on white paper than that made by one drop of the first tint with forty-six drops of water could not have been seen. On the other hand, when $t = 12$, the difference of the angle of visibility from that for absolute blackness is only $0'' \cdot 1$. The constants in this and the other formulæ will depend of course on various circumstances of illumination, the state of the individual eye, &c.

Mayer made a series of observations with several parallel lines drawn with China ink on white paper (well stretched), the width of the lines and intervals in one case being 0·032 of an English inch (0·36 *de ligne*). These lines he could perceive to be several at 11 feet (*pieds de Roi*) distance with the light from an open window to north, or when the angle subtended by the interval was 47″. He then made observations with the same lines lighted by a wax candle placed at different distances from them.

I give Mayer's observations for this set of lines here for comparison with the preceding results for different tints.

D.	d.	α. Observed.	Angle α, calculated by (3).	Error.	(5).	Error.	(6).	Error.
feet.	feet.	″	″	″	″	″	″	″
7·47	0·5	69	63	− 6	66	−3	69	0
6·53	1·0	79	79	0	78	0	79	0
5·73	2·0	90	99	+ 9	96	+6	92	+2
4·73	3·0	109	114	+ 5	108	−1	103	−6
4·48	4·0	115	125	+10	118	+3	112	−3
3·51	8·0	147	158	+11	146	−1	141	−6
3·00	13·0	172	185	+15	172	0	172	0

Mayer considered that he explained his result by supposing the limiting angle (α) of distinct vision to be as the cube root of the distance of the candle from the paper, or

$$\alpha = 79 \sqrt[3]{d}, \qquad (3)$$

where α is in seconds and d is in *pieds de Roi*. He also arrived at the following curious conclusion. Since the limiting value of α for the

same lines in full daylight was 47″, by substituting this value in equation (3), he found $d=0{\cdot}2$ foot; and "we may conclude," he says, "that the light of day is as strong as that of a candle at one fifth of a foot from the object. Consequently if we wish to light an object with a candle as strongly as it would be by daylight [and even by strong sunlight, as Mayer found], we must employ twenty-five lighted candles placed at a distance of one foot from the object"* !

Had Mayer's formula been an exact representation of the observations, he might have concluded that the eye could separate parallel lines as well with twenty-five candles at one foot distance as with full sunlight; but it will be seen that the errors of the formula increase as d increases and diminishes. An equation of the form given by Mayer which best represents the observations is found by least squares to be

$$\alpha=77{\cdot}6\,d^{\frac{1}{2{\cdot}57}}\,;\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(4)$$

but this also fails when d is small. The following equation best represents Mayer's observations, including that for daylight:—

$$\alpha-47=25\sqrt{d}+30\log(d+0{\cdot}9).\ \ldots\ldots\ldots\ldots\ldots(5)$$

When $d=0$, $\alpha=48''{\cdot}3$. The errors of this equation are given in the preceding Table. But it will be seen here also, from the distances D of the observer (which I have calculated from the angles given by Mayer), that when the distances d of the candle increase in a geometrical progression, the distances D diminish in an arithmetical progression as nearly as the accuracy of the observations admit. This fact I have myself verified by repeating Mayer's observations. We may then represent the observations by an equation of the same form as the others. The following has its computed values and errors given in the Table:—

$$\alpha=\frac{79}{1-0{\cdot}485\log d}\cdot\ldots\ldots\ldots\ldots\ldots\ldots(6)\dagger$$

It follows from this formula that when the candle is 140 feet from the paper, the eye at 8 inches from it could just see the lines and spaces; when $d=\frac{1}{25}$ foot, $\alpha=47''$, the smallest angle under which the lines and spaces can be seen.

It might have been supposed that the distance of the observer from the paper would vary inversely as the illumination, or that α should vary as d^2, which, it will be seen, is very far from being the case.

The 7th observation previously given represents more nearly the case of the examination of test-lines on glass, the spaces being equally bright, or nearly so, in all cases, while the lines have a variable depth of shade. In Mayer's observations both spaces and lines receive less light as the candle is removed. The impression on the retina for the separation of

* Pezenas, 'Cours complet d'Optique,' t. ii. p. 415.

† Or, $D=6{\cdot}53\,(1-0{\cdot}485\log d)$.

the lines receives no improvement in the first case by any increase of depth of tint of the lines beyond a certain feeble shade, nor in the second (Mayer's) case by any increase of the illumination of both lines and spaces beyond that of a candle held near.

8th observation.—It was sought whether the visibility of parallel lines increased with their length, as in the case of single lines. Four long parallel lines having been drawn, of the width and at the interval of 0·048 inch, on a sheet of white paper, this was pinned, as before, to a smooth plank and placed in the open air in the shade (before sunset); the lines were covered by a sheet of the same paper so as to show variable lengths. The following Table contains the results of the observations:—

l.	D.	α.	(7).	Errors.	β.
in.	ft. in.				
0·4	16 8	49·5″	49·5″	0·0″	413″
0·8	17 4	47·6	47·4	−0·2	794
1·6	18 5	44·8	45·5	+0·7	1490
3·2	18 10	43·8	43·8	0·0	2921
6·4	19 5	42·5	42·1	−0·4	5664

D is the distance of the observer, l the length of the lines, β is the angle subtended by the length at the eye of the observer; α, the angle subtended by the width of the lines, is represented nearly by the following formula:—

$$\alpha = \frac{352}{\log l + 4{\cdot}53}, \qquad (7)$$

where l, the length of the lines, is expressed in units of 0·001 inch. The variation of the angle is comparatively small in this case. The law of variation of α seems to change when the length of the lines becomes less than the width of the whole.

9th observation.—The following observations were made with short parallel lines drawn *separately* on the same sheet, with the same width of intervals and lines as in the last case:—

l.	D.	α.	(8).	Errors.	β.	$\alpha\sqrt[3]{\beta}$.
in.	in.					
0·40	204	48·6″	48·0″	−0·6″	404″	359
0·20	186	53·2	53·5	+0·3	222	322
0·10	168	58·9	60·6	+1·7	123	293
0·05	144	68·8	69·9	+1·1	72	286
0·025	120	82·5	82·5	0·0	43	289
0·012	96	103·1	101·0	−2·1	26	306

The observations are represented nearly by the equation

$$\alpha = \frac{137}{\log l + 0{\cdot}27}. \qquad (8)$$

It will be seen tha tthe angle α increases rapidly as the length dimi-

nishes; on the other hand, β, the angle subtended by the length of the lines, decreases much more rapidly. In this case, as in that of the 4th observation, we find a and β are connected nearly by the following formula:—

$$a\sqrt[3]{\beta}=\text{constant} \qquad (9)$$

10*th observation.*—The long parallel lines were seen by different persons at a greater distance when inclined to the horizontal by an angle of about 25° below it to the right and above it to the left, but the visibility varies at different angles for different persons.

11*th observation.*—Jurin's observations of the difference of visibility of parallel lines and a single line had reference to the case of only two black lines with a white line between. On comparing the distances at which parallel lines could be seen 0·4 inch long and of the same width and interval as before (0·048 inch), it was found that it was most difficult to separate the lines when there were only two. The following are the observations made:—

Number of lines.	D.		a.
	ft.	in.	″
1	50	0	16
2	13	4	62
3	15	0	55
4	17	2	48

As my object at present has been simply to state the facts observed, I shall now proceed to my examination of the photographs of Nobert's test-bands. The use of these I owed to the kindness of Mr. Eulenstein (a histologist well acquainted with the microscope and its tests), who informed me that these lines were made with the same diamond-point, but that the pressure on the point is made to increase as the number of lines to the inch diminishes. The widths of the lines and of their interspaces were measured by me by means of a glass scale accurately divided with one thousand divisions to the inch, the dividing lines being about one tenth the width of the interspaces. This scale was placed one half on thin white paper, the other covering the photographed lines. The readings were made with the aid of a pocket-lens of 1·1 inch focal distance. The width of every line and interspace was measured for the first six bands; from eight to ten lines and spaces were measured from the VII.th to the XIII.th bands inclusive; in each case the means of these measures are given. For the remaining bands the number of lines and their spaces were counted and their whole width observed to the XVII.th band. In the XVIII.th and XIX.th bands the lines run into each other in different places (in some of the previous bands the lines occasionally fail). It was evident that for the highest bands the machine failed to make the number of separate lines which were drawn. In several cases diffraction-fringes interfered with the accuracy of the measures; but as these were generally made

from bands near the middle of the photographs, there could be no doubt whether the line was a fringe or not.

The following Table contains the results of these observations :—

Measures of Nobert's Test-lines.

Band.	Number of lines.	Width of			Number to the inch.			Ratio.
		Line.	Space.	Band.	Lines.	Spaces.	Both.	
I.	7	28·57	58·67	553	35,000	17,040	11,460	1·00
II.	10	35·10	23·00	560	28,500	43,450	17,210	1·68
III.	13	27·85	14·85	555	35,910	67,340	23,420	2·11
IV.	15	19·13	15·57	505	52,270	64,230	28,820	3·10
V.	17	14·10	15·20	480	71,430	65,790	34,250	3·87
VI.	20	11·55	13·90	495	86,580	71,940	39,290	4·23
VII.	23	12·64	9·14	505	79,110	109,410	45,910	4·65
VIII.	25	8·71	10·71	475	114,810	93,370	51,390	5·49
IX.	28	7·47	9·93	478	133,870	100,700	57,470	5·92
X.	30	8·11	7·44	460	123,300	134,410	64,310	7·25
XI.	34	8·10	6·90	500	123,300	144,930	66,670	7·25
XII.	37	6·81	7·28	503	146,840	137,360	70,970	8·08
XIII.	40	6·62	6·00	500	151,060	166,670	79,240	8·89
XIV.	43	6·00	6·00	510	166,670	166,670	83,330	9·81
XV.	45	5·56	5·56	495	179,860	179,860	89,930	10·50
XVI.	(40)	......	......	522	....	....	77,280	
XVII.	(40)	......	......	$\begin{Bmatrix} 503 \\ 512 \end{Bmatrix}$	....	....	77,880	
XVIII.	(40)	......	......	$\begin{Bmatrix} 489 \\ 511 \end{Bmatrix}$	....	....	79,240	
XIX.	(40)	......	......	540	....	....	75,760?	

Notes.—These measures are frequently mere approximations ; and in several bands the graving-point has made a wonderful approach to an equality of width of lines and spaces ; indeed these lines are marvels of mechanical skill. If, in the case of each band, the first and last lines had been drawn longer than the rest, it would have been possible to measure the width of a line with considerable accuracy, since, as has been shown, the visibility of a single line is nearly twenty times that for the series.

The widths of the lines and spaces are those taken from the photographs, the unit being $\frac{1}{1000}$ inch. The photographs are magnified to 1000 times. In bands XVII. and XVIII. second measures are given from photographs magnifying to 1600 times (but reduced to the same unit). The number of lines in ( ) are the numbers counted for which the total width was measured. The number for the XIX.th is deduced from the measure of a few where the lines were most distinct. The numbers of lines and spaces to an inch are the numbers which could be put in an inch laid side by side (without interval). Under " Both " is given the number of lines to the inch (with interspaces), as in the bands. The "Ratio" is that of the number for the widest space (17,000 to the inch) to the number for the widest line or space in the following bands.

It will be seen that the least width of the lines which can be counted and measured on the photographs is about $\frac{1}{160,000}$ of an inch (XIII.th band). We have seen (5th observation) that dark parallel lines on glass can be seen with transmitted light when their width subtends an angle of 20″ to 26″; so that lines stopping the light moderately (7th observation) of $\frac{1}{160,00}$

of an inch wide should be seen with a power of 125, and counted with a power of 160 (the distance for the unaided eye being considered 8 inches). We have, however, obviously in the high bands to include the case of observation 7, the lines on the photographs being excessively faint. When we add to this fact (a most important one when such lines are supposed to give some measure of the power of the microscope) that it appears that separate lines cannot be drawn of a less width than about $\frac{1}{180,000}$ of an inch under the diminished pressure of Mr. Nobert's machine without the graving-point sliding into previous grooves, we have a sufficient explanation why the power of the microscope cannot be measured by these lines.

The following are the conclusions of this note :—

1st. That lines can be seen by the naked eye with transmitted light the width of which subtends an angle of about 1″ (2nd observation).

2nd. That the visibility of a line, or the distance at which it can be seen, depends on the logarithm of its length, the product of the angle subtended by the width and the cube root of that subtended by the length being nearly constant (4th observation).

3rd. Short parallel lines could be seen by transmitted light when the angle formed by the width of the spaces and intervals was 20″ (5th observation).

4th. The visibility of lines of the same width increases as the distance between them decreases (6th observation).

5th. The visibility of parallel lines depends on the darkness of the shade or tint of the lines up to a certain feeble tint, after which no blacking of the lines increases the visibility; the distance to which the lines can be seen depends on c^t, where c is a constant and t is the number of the tint or shade (the number of coats of a weak tint) (7th observation).

6th. The visibility of dark parallel lines lighted with a candle depends on the logarithm of the distance of the candle from the lines; and they can be seen as well with a candle placed quite near as with the strongest daylight. This results from Tobias Mayer's observations.

7th. The visibility of parallel lines depends on the logarithm of their length, as in the case of single lines, the variation being much greater for short parallel lines than for long ones. Also for short parallel lines the product $\alpha\sqrt[3]{\beta}$ is nearly constant, as for single lines (see second conclusion).

8th. Parallel lines are least visible when there are only two, and increase in visibility with their number.

9th. Nobert's test-lines fail as a test for the microscope, especially in the highest bands, from the incapacity of the machine to make separate lines at less intervals and of less width than $\frac{1}{160,000}$ of an inch; they also fail, in all probability, on account of the faintness of the tint or shade of the lines made on the retina.

XI. "On the Change produced by Magnetization in the Electrical Resistance of Iron and Steel.—Preliminary Notice." By Professor W. G. Adams, F.R.S. Received June 1, 1875.

For some time past Mr. Herbert Tomlinson, Demonstrator in the Physical Laboratory of King's College, has been engaged in carrying out a series of experiments on this subject, and also on the effect of change of tension on the electrical resistance of steel and iron wires.

In measuring the resistances of the short lengths of the wires or rods which were employed, a unit was chosen which was a small fraction of the British-Association unit.

Experiments were made with rods of soft iron about one eighth of an inch thick, with soft steel, and also with steel of different degrees of hardness.

With a rod of soft iron about 3 feet long there was an increase of resistance of about 1 per cent. on magnetizing with two Grove's cells. The whole resistance of this rod was 32 units.

The experiments were repeated with the rod placed in ice and also in water at the ordinary temperature (about 15° C.), and with nearly the same change in the resistance of the rod. The change in the temperature of the water was found to be about 1° C. during the experiment.

Another rod of soft iron was employed whose resistance was 50 units. The magnetizing current was measured by means of a tangent-galvanometer, and the resistance was measured by means of Wheatstone's bridge. There was found to be an increase in the resistance of the rod when it was converted into a magnet by sending the magnetizing current through a wire which was coiled round it in the form of a spiral.

It was found that the electrical resistance was increased when any addition was made to the strength of the magnetizing current. When the increase in the electrical resistance was divided by the square of the strength of the magnetizing current, a series of numbers was obtained which did not differ much from one another; the values of these numbers mostly lie between 3 and 4.

When the magnetizing current is considerably increased, the ratio of the increase in the resistance to the square of the magnetizing current diminishes rather rapidly.

A similar series of experiments was made with a thick knitting-needle made of *soft steel*. The resistance of the needle was 29 units. In this case also the resistance was found to *increase* when the strength of the magnetizing current was increased. On dividing the increase of resistance by the square of the magnetizing current, the numbers obtained from a considerable number of experiments lie between 4·7 and 5·6, showing that the ratio of the increase of resistance to the square of the magnetizing current is very nearly constant.

When the magnetizing current is considerably increased, this ratio is found to diminish, just as in the case of soft iron.

Different kinds of *hard* steel were tried.

(1) An ordinary knitting-needle, of which the resistance was 66·5 units.

On magnetizing with currents of different strengths, there was found to be a diminution in the resistance; and it was also found that the diminution of resistance increased when the strength of the current was increased. With currents varying from tan 15° to tan 54° 30′ the diminution amounted to 4·33, *i. e.* about 6·5 per cent. of the whole resistance. The temperature *increased* about 2° C. during the experiment.

Dividing the loss of resistance by the square of the magnetizing current, the results of four sets of experiments gave the following values :—

$$\frac{\cdot 165}{(\tan 15°)^2}=2\cdot 29,$$

$$\frac{\cdot 7525}{(\tan 30°)^2}=2\cdot 26,$$

$$\frac{2\cdot 3225}{(\tan 45°\ 30')^2}=2\cdot 24,$$

$$\frac{4\cdot 330}{(\tan 54°\ 30')^2}=2\cdot 21.$$

Four Grove's cells were employed for the strongest current.

Two other experiments which had been tried previously gave results 2·30 and 2·26 for the ratio of the diminution of resistance to the square of the magnetizing current, thus showing that the diminution in the resistance is almost exactly proportional to the square of the current.

The diminution in resistance does not take place all at once, but gradually, and also ceases gradually when the current is stopped.

(2) A steel needle was also magnetized longitudinally by placing it on a copper strip at right angles to the lines of force of a current across the strip.

There was found to be diminution of resistance on increasing the current. The values obtained from two series of experiments were :—

$$\frac{\cdot 064}{(\tan 8°\ 30')^2}=2\cdot 88,$$

$$\frac{\cdot 192}{(\tan 15°)^2}=2\cdot 63.$$

With stronger currents this ratio was found to diminish.

On magnetizing the wires transversely by sending a current in the direction of their length, a diminution of resistance was also observed, which diminution also increased when the strength of the current was increased.

When a current was sent along the wire itself, on increasing the

current there was found to be also a diminution of resistance in the case of hard steel, and an increase of resistance in the case of soft iron and soft steel.

Thus the effects produced are the same as those due to transverse magnetization by a neighbouring current.

Conclusions to be drawn from the experiments:—

(1) The effect of passing any current through a bar of hard steel is to diminish its resistance, and through a bar of soft iron or soft steel is to increase its resistance.

(2) When a bar of hard steel is magnetized by sending a current through a coil which encloses it, there is a diminution of resistance which is directly proportional to the square of the magnetizing current up to a certain limit.

(3) When soft steel or soft iron is magnetized longitudinally or transversely, there is an increase of resistance which is nearly proportional to the square of the magnetizing current.

XII. "The Action of Light on Selenium." By Prof. W. G. Adams, M.A., F.R.S. Received June 17, 1875.

(Abstract.)

The paper contains an account of several series of experiments made in December and January last on this subject with the view:—

(1) To determine whether the change in the electrical resistance of the selenium is due to radiant heat, light, or chemical action.

(2) To measure the amount of the change of resistance due to exposure to light from different sources and through various absorbing media.

(3) To determine whether the action is instantaneous or gradual, and, if possible, to measure the rate at which the action takes place.

The selenium formed one of the four resistances in a Wheatstone's bridge, and its average resistance was about $2\frac{1}{2}$ megohms.

The two resistances in the bridge, which were kept constant, were 4 and 2000, so that the resistance of the selenium was 500 times the variable resistance required to balance it.

R is taken to represent this resistance required to balance the selenium. The box containing the selenium was laid on its side and had a draw-lid, which was kept closed except when exposure was made. In front of the draw-lid was a black screen with an opening opposite to the selenium 6 centims. by $3\frac{1}{2}$ centims., into or in front of which various absorbing media could be placed.

The absorbing media employed were bichromate of potash, sulphate of copper, ruby, orange, green, and blue glasses. Plates of rock-salt, alum, mica, and quartz were also employed.

With the lid of the box on, the resistance of the selenium was measured, and was found to increase slowly and regularly in consequence of the

heating by the current. In most of the experiments a battery of 30 Leclanché cells was employed.

It was found that the higher the battery-power the less is the resistance of the selenium. Experiments with 5, 30, and 35 cells gave the following results :—

Resistance R with	5 cells	....	5400 ohms.
,, ,,	35 ,,	....	4400 ,,
,, ,,	5 ,,	....	5400 ,,
,, ,,	30 ,,	....	4600 ,,

After some hours :—

Resistance R with	30 ,,	....	4800 ,,
,, ,,	5 ,,	....	5750 ,,

This diminution of resistance with increased battery-power may be accounted for *in part* by leakage from the rheocord ; and there may be also an opposing electromotive force similar to polarization brought into action in the selenium when the current is passing which increases with the current.

Exposure to light diminishes the resistance of selenium.

This may be accounted for by either of two hypotheses :—

(1) That light acting on the selenium sets up a polarization current in it which opposes the battery-current passing through it.

(2) That light makes the selenium a better conductor of electricity by producing a change in its surface similar to the change which it produces on the surface of a phosphorescent body, by which that body is enabled to give out light after it has been exposed.

With the same battery-power, an increase in the temperature causes an increase in the resistance of the selenium.

In December, before the extreme cold, the resistance R with 30 cells was about 5200. Throughout the extreme cold, from December 18 to January 4, the resistance R was about 4400. This was the value of R at 1 P.M. on January 1st, a bright cold day. On January 5 the temperature out of doors changed to 44° F. in the shade at 12 o'clock, and the value of R was found to be 5400. These values were obtained before the box was opened, and were the first experiments made on the days named.

When first exposed after being closed up for some days or even hours, the selenium is more sensitive to light : this sensitiveness increases with the time during which the selenium has been kept in the dark ; hence the first experiment is generally not comparable with the others.

On exposure to light the resistance is diminished ; but on being again eclipsed, the selenium returns in a very few minutes *nearly* to its previous resistance.

The change of resistance produced by exposure to daylight sometimes amounts to one fourth of the whole resistance of the selenium.

The experiments with various absorbing media seem to show that the

action through media which absorb all the more chemically active rays is very nearly as great as when they are not interposed, so that the chemical rays produce very little effect.

Experiments with the lime light, with rock-salt, alum, and quartz, and their combinations, two together, show that the resistance diminishes at the same rate as the illumination increases. This seems to show that the action is almost entirely due to the illuminating power of the light falling on the selenium.

Experiments with the electric light, with smoked rock-salt, alum, and a solution of iodine in bisulphide of carbon show that the obscure heat-rays do not act powerfully on the selenium.

In one series of experiments an attempt was made to separate the instantaneous effect from the gradual effect of the light.

This was done by first balancing the resistance of the selenium before exposure by a resistance R of the coils, then diminishing R by 300, 400, or 500 ohms, according to the brightness of the light, so as to get no sudden deflection when the current is made at the first instant of exposure.

It was difficult to determine beforehand by estimation what diminution of R should be made; but after several trials it was quite possible to make the sudden deflection very small, either on one side of the zero or the other, and to keep the needle near the zero by continuing to diminish the value of R as long as the exposure lasted.

In this way the effects of exposure in successive equal intervals of time can be measured.

The light allowed to pass through the coloured glasses and other absorbing media was examined by a spectroscope, and it was found that the yellowish-green rays were among the most active in altering the electrical state of the selenium.

A series of experiments was made to determine the effect of light from different sources.

A Bunsen burner was employed, and chloride of barium, chloride of strontium, thallium, and sal-ammoniac were introduced into the flame.

The effect with barium seemed to be less than with strontium.

With sal-ammoniac in the flame the effect was as great as with strontium and more lasting.

With thallium the effect was considerably greater, more gradual, as well as far more lasting than with strontium.

The effect on repeating an experiment is very much less than the effect of the first exposure with each new source of light.

Experiments were made with the Bunsen burner alone in its ordinary state and when it is rendered luminous by stopping the air-holes.

Exposure to the ordinary Bunsen flame for several seconds only caused a slight deflection of about 10 divisions of the scale. After this slight diminution of resistance the needle gradually returned to zero, and was

deflected to the other side, as the heat radiated from the Bunsen burner was absorbed by the selenium.

On making the flame luminous, the needle was suddenly deflected off the scale with great rapidity.

With the 10 shunt to the galvanometer there was no deflection on exposure to the ordinary Bunsen flame; but with the luminous flame there was a sudden deflection, which increased to 250 divisions of the scale in a few seconds.

This corresponded to a change of resistance in R of about 1250 ohms.

This experiment was repeated in a slightly different way. The selenium was balanced, and before exposure to the *luminous* Bunsen flame, R was diminished by 1000 ohms. On making contact and exposing at the same instant, there was a slight deflection, showing that the sudden effect was equivalent to rather less than 1000 ohms; but in a very few seconds the needle was at rest at zero, and to keep the needle at zero the resistance was further diminished by 300 ohms.

The resistance had been diminished by one fourth of its whole amount in less than one minute in consequence of the exposure.

Exposure to an ordinary wax taper diminished the resistance of the selenium by 300,000 ohms, or about one eighth part of its whole resistance.

The illuminating powers of these sources of light were compared by means of the Bunsen photometer.

The light of the ordinary Bunsen flame could scarcely be measured, but was somewhere about $\frac{1}{200}$ part of a candle, and of the luminous Bunsen flame about 10 candles, whilst the light from the taper was at its best rather more than one fourth of a candle.

The heating effects of these three sources were compared by means of the thermo-electric pile and delicate astatic galvanometer.

At a distance of one foot from the face of the pile the deflection produced by the ordinary Bunsen flame was $46\frac{1}{2}°$, and by the luminous Bunsen flame was 52°, whilst the taper produced no effect which could be measured.

These experiments clearly show that very little effect is produced by the radiation of obscure heat, but that the effect is due almost entirely, if not entirely, to light.

As the effects produced were measured in deflections of the needle, some series of experiments were made to determine the value in resistances equivalent to the divisions of the scale, from which it appeared that with the 10 shunt to the galvanometer and with 30 cells, 20 divisions of the scale were equivalent to 100 ohms resistance; and without the shunt, 100 divisions of the scale were equivalent to from 100 to 110 ohms.

Experiments were also made to determine whether moonlight would produce any change in the electrical resistance of selenium. The experi-

ment was made at the half-moon, when the moon was high up, so that the light fell obliquely on the window and did not shine directly on the selenium.

On throwing the moonlight on the selenium by means of a plane mirror, the needle was at once deflected 20 divisions of the scale; on placing the mirror outside the window so as to send the moonlight perpendicularly through the window on the selenium, the deflection of the needle was 40 divisions. The window was kept closed during these experiments.

On another evening when the moon shone very obliquely on the window, and the selenium was exposed on the inside of the window directly to the moonlight, the needle was deflected 100 divisions of the scale, and the deflection increased to 150 divisions after exposure for about 3 minutes.

The change in the resistance of the selenium was from 60,000 to 70,000 ohms.

These experiments show that the action on the selenium is due principally, if not entirely, to radiations belonging to the visible part of the spectrum. Light rays of all kinds, particularly the greenish yellow, produce an instantaneous effect followed by a more or less gradual effect, which continues to increase during exposure for several minutes.

These facts suggest two hypotheses as possible explanations, which may help as guides in further experiments, but which cannot be accepted as proved without further evidence.

(1) That the light falling on the selenium causes an electromotive force in it, which opposes a battery-current passing through it, the effect being similar to the effect due to polarization in an electrolyte.

(2) That the light falling on the selenium causes a change on its surface akin to the change which it produces on the surface of a phosphorescent body, and that in consequence of this change the electric current is enabled to pass more readily over the surface of the selenium.

XIII. "On the Production of Glycosuria by the Effect of oxygenated Blood upon the Liver." By F. W. Pavy, M.D., F.R.S. Received June 17, 1875.

In a communication on "Lesions of the Nervous System producing Diabetes," presented to the Royal Society in 1859 (Proc. Roy. Soc. vol. x. 1859–60), I made known that division of certain parts of the sympathetic system occasioned the presence of sugar in the urine. The effect of puncturing the floor of the fourth ventricle (Bernard's celebrated experiment) had been for some time previously familiar to physiologists; but nothing had been ascertained about the production of diabetes by lesions of the sympathetic, until my experiments upon the subject were conducted; and it was in attempting to discover the channel through

which the medulla oblongata influenced the liver, that I was led to recognize the facts which my researches disclosed.

These results merely showed that there were other means besides puncturing the floor of the fourth ventricle by which artificial diabetes could be induced. They did not explain the reason of the appearance of sugar, and I still sought to discover something upon this point. Failing to reconcile any explanation that has been suggested with the evidence furnished by experiment, I have from time to time pushed inquiry in various directions, but always with a fruitless issue, until the summer of 1874, when I came across the results which it is the object of this communication to make known, a brief announcement of them having been previously made in a letter to the Secretaries of the Royal Society shortly after the close of the session of last year.

For some time past I have been led to look to an altered condition of the blood flowing to the liver as likely to prove the most probable cause of the transformation of amyloid substance into sugar, which evidently constitutes the foundation of the artificial diabetes following operations upon the nervous system. Schiff is of this view, and (' Journal de l'Anatomie et de la Physiologie,' Paris, 1866) has referred the escape of sugar from the liver, and thence the production of glycosuria, to the development of a ferment in the blood as a result of the hyperæmia which follows the operations on the nervous system which occasion artificial diabetes. It is not necessary, according to his view, that there should be hyperæmia specially of the liver; but hyperæmia anywhere may lead to the development of the ferment which he alleges to be the source of the appearance of sugar. "Le diabète par suite de l'hyperémie pourrait donc bien ne pas être l'effet spécifique d'une hyperémie du foie, mais de chaque hyperémie générale d'une certaine étendue." I have carefully examined this view of Schiff, and cannot obtain evidence of the development of a ferment in the manner asserted; moreover I have tried the effect of introducing a secretion, viz. saliva, into the circulatory system, which is known to act as an energetic ferment upon the amyloid substance of the liver. At first I introduced it into the general circulation through the jugular vein, and failed to observe the production of saccharine urine. Later on, thinking the experiment might be more effective, I injected it into a branch of the mesenteric vein, so that it would all pass directly to the liver. Upon one occasion I found that the urine, from some cause or other, became to a moderate extent saccharine; but in a large number of other experiments the operation was attended with a negative result. The quantity used varied in different instances, the largest amount employed being 13 fluid drachms. In some cases the saliva was introduced in a pure state, in others after dilution with varying proportions of water.

Looking at the fact that the amyloid substance is contained in the hepatic cells, it is scarcely surprising that a ferment of a colloid nature

contained in the blood should fail to be capable of determining the production of sugar whilst passing through the vessels of the liver.

Having so far proceeded without success, it now occurred to me to try the effect of introducing defibrinated arterial blood into the portal system. I was led to experiment in this way from having a long time previously observed that when arterial blood only was allowed to flow through the liver (as, for instance, when the portal vein was tied and the hepatic artery left free), sugar escaped from the organ to such an extent as to render the contents of the circulatory system strongly saccharine. This result I had commented upon as being somewhat surprising, and as furnishing evidence standing in opposition to Bernard's glycogenic theory. I had not succeeded by the operation in producing glycosuria, because, as it appeared to me, no urine was secreted, owing to the ligature of the portal vein leading to such a diversion of blood from the general circulation, by the accumulation occurring in the portal system, that the flow through the kidney was too slight to allow of it. I had endeavoured to overcome this obstacle by connecting, through the medium of a canula, the portal with the right renal vein after ligaturing the corresponding renal artery. If the experiment had succeeded, the liver would have been left with its arterial supply, but the portal stream would have been diverted and made to reach the inferior cava without traversing the hepatic vessels. As regards the operation, this I found I could accomplish; but each time I performed the experiment the object I had in view was frustrated by the canula becoming quickly filled with a plug of blood-clot. It was whilst under this difficulty that I thought of collecting blood from an artery, defibrinating it, and then introducing it into the portal system. I had considered it possible that some slight effect might be perceptible, but had not anticipated the strongly marked result which is producible.

The amount of blood used has been from 10 to 18 fluid ounces. After the production of anæsthesia by chloroform the blood was collected from the carotid artery, stirred in order to defibrinate it, strained, and then very slowly injected into a branch of the mesenteric vein. In one experiment, where half an hour had been employed in making the injection, the urine, at the completion of the operation, contained a notable amount of sugar, and half an hour later showed, by analysis, the presence of 15 grains to the fluid ounce. In a second the urine contained 10 and in a third 14 grains to the fluid ounce when collected three quarters of an hour after the operation.

The experiments were performed upon dogs, and in each case it had been ascertained that the urine was devoid of sugar before the operation.

It will thus be seen that these results leave no doubt about the decided production of glycosuria. The effect was not only rapid but of a strongly marked character. It is necessary, however, before concluding that the glycosuria was really attributable to the influence of the oxygenated blood, to have evidence that in the absence of oxygenated blood a

negative result would be obtained. I took care, by slowness of injection, to avoid allowing the circulation through the liver to be influenced by the effect of increased pressure within the portal system. I am not aware, and do no think from my previous experience, that any fallacy would be likely to arise from such a source; but I nevertheless deemed it advisable to exclude the possibility of its occurrence.

Having noticed the effect which has been described from the injection of oxygenated blood into the portal system, it became necessary to ascertain positively that it was attributable to the oxygenated condition of the blood, and not to any other cause. To decide this point an appeal to the counterpart experiment was made. Defibrinated venous instead of arterial blood was injected into a branch of the mesenteric vein; and upon each occasion where such an operation has been performed a negative result has been obtained. With the evidence thus furnished the conclusion may be warrantably drawn, that oxygenated blood in some manner influences the liver, so as to lead to the production of glycosuria. It may be inferred that, contrary to the effect of venous blood under a natural state of the circulation, it promotes the transformation of amyloid substance into sugar.

In performing these experiments of injecting defibrinated blood into the portal system, I came across an effect which I had not anticipated, and which upon several occasions frustrated the object I had in view. Defibrinated blood, as experimental physiologists know, may be injected into the veins of the general system without producing any special effect. Injected, however, into the portal system, it frequently gave rise to a complete stoppage of the portal circulation by inducing coagulation of the blood in the trunk of the portal vein and its ramifications in the liver. I merely mention the fact in this place, and am not aware that it has hitherto been noticed; but it may hereafter be found of interest in connexion with the subject of the coagulation of blood. Finding that several of my experiments proved fruitless from this cause, I tried means to obviate coagulation occurring, and amongst these was extirpation of the spleen. With this operation, as far as my experience has yet gone, the coagulation is prevented; and, moreover, I have noticed that the blood in the general system after death fails to possess its ordinary coagulating property.

The suggestion naturally occurs, that what has been stated in this communication affords an explanation of the glycosuria occurring after Bernard's puncture of the fourth ventricle and the various lesions of the sympathetic. Without any new agent being called in, sufficient is presented in the state of the blood to account for the production of sugar that occurs. By a vaso-motor paralysis affecting the vessels of the chylopoietic viscera the blood will reach the portal system without having become dearterialized in its natural way; and in this state it has been shown to possess the property of acting within the liver in such a manner as to determine the production of glycosuria.

XIV. "On some supposed changes Basaltic Veins have suffered during their passage through and contact with Stratified Rocks, and on the manner in which these Rocks have been affected by the heated Basalt." By I. Lowthian Bell, F.R.S. Received May 27, 1875.

The northern counties of England afford very satisfactory evidence of the intrusion, in former geological times, of a large area of fused matter beneath portions of the then-existing surface, and through the vertical, or nearly vertical, faults and fissures of thin sedimentary strata.

Through the observations and writings of N. J. Winch*, Sir Walter Trevelyan†, Prof. Sedgwick‡, William Hutton§, John Buddle‖, Nicholas Wood¶, Westgarth Forster**, and several other observers of earlier†† and more recent‡‡ date, we have been made acquainted with many details of the existence, appearance, and direction at the surface of extensive whin-dykes and beds of intercalated trap, basalt, or whin, using the terms which have hitherto been generally and locally applied to the igneous rocks found in connexion with the Carboniferous formation of these districts.

In the most northerly part of Northumberland a broad dyke of igneous rock occurs on Holy Island, and is continued on the mainland to the west. Several other dykes, too numerous, indeed, to be specially mentioned in this communication, occur in the Carboniferous rocks between this locality and the banks of the Tees, having for the most part a direction from west to east. In addition to the dykes which may have filled up old lines of faults and fissures, we have bedded igneous rocks, where fused matter, instead of coming to the surface, has forced its passage or way, horizontally between the regular and previously stratified sedimentary rocks. Sometimes also the igneous matter is found between the central portions of individual beds of shale and limestone, and in its course has oftentimes enclosed in its mass considerable portions of such preexisting beds, as in the examples figured by Prof. Sedgwick in the second volume of the Cambridge Philosophical Transactions.

* Trans. Geol. Soc. 1814, vol. iv. pp. 21 & 73.

† Wernerian Soc. Memoirs, 1821–23, p. 253, and Trans. Nat. Hist. Soc. Northumberland and Durham, 1830, vol. i. p. 58.

‡ Cambridge Phil. Trans. vol. ii. pp. 21 & 139.

§ Trans. Nat. Hist. Soc. Northumberland and Durham, 1831, vol. ii. p. 187.

‖ *Ibid.* 1830, vol. i. p. 9.

¶ *Ibid.* 1831, vol. i. p. 327.

** Section of the Strata &c., 1821.

†† Hon. H. G. Bennett, M.P., F.R.S., Geol. Trans. 1812, vol. iv. p. 102; Conybeare and Phillips, Geol. of England and Wales, 1821, pt. 1; Michael Forster, Trans. Nat. Hist. Soc. Northumberland and Durham, 1830, vol. i. p. 44; Francis Forster, *ibid* p. 75; Henry T. M. Witham, *ibid.* vol. ii. p. 343.

‡‡ George Tate, Trans. Tyneside Nat.-Hist. Field-Club, vol. ii. new series.

The Whin-sill, as it is termed by the lead-miners of the Alston-Moor district, is the most remarkable example of bedded igneous rock in the northern counties, extending as it does from the Farne Islands in the north to the county of Northumberland; and after passing the Stublick Dyke, by which it is faulted, it skirts the escarpment of the Pennine range in its outcrop, and terminates, so far as we are informed, in the neighbourhood of Lunedale in Yorkshire.

There is another but smaller occurrence of bedded basaltic rock near Stanhope, in Weardale, which was formerly, and very accurately, described by Sir Walter Trevelyan in the first volume of the 'Transactions of the Natural-History Society of Northumberland and Durham.'

Another lateral intrusive mass of whin exposed by denudation at the surface occurs at Bolam, in South Durham, in connexion with the celebrated Cockfield-Fell Dyke. This mass has been figured and graphically described by Prof. Sedgwick in the Cambridge Transactions above quoted.

A further instance of the lateral or horizontal intrusion of igneous rock which we have now to describe occurs in connexion with the basaltic dyke which extends from Egglestone Moor along the Bedburn Beck, through Bitchburn Colliery, Constantine Farm, Whitworth, Tudhoe, Hett, Tursdale, and Crow Trees, to Quarrington Hill, on the escarpment of the Magnesian Limestone. This dyke was first described, and its direction traced, by Prof. Sedgwick. Though it is nowhere seen in contact with the Magnesian Limestone, its existence is proved in the colliery of Shotton, which is worked entirely under that formation.

At the point where the Hett Whin-dyke crosses the Wear are the workings of Pagebank Colliery, which is in the occupation of Messrs. Bell Brothers. A little more than two miles to the north is a second basaltic dyke, running parallel to the Hett Dyke for some distance; but I am not aware that any actual point of junction has hitherto been observed between these neighbouring dykes*.

Connected with the mining-operations of the above-named colliery a bore-hole, to prove the position of the coal, was commenced about 730 yards to the south of the more northern dyke. The usual well-known strata of that district were passed through; but on reaching a depth of 56 fathoms a hard rock was struck, which proved to be whinstone. Fourteen weeks were consumed in penetrating 26 inches into this obstacle, after which it was abandoned; and a second hole was commenced 940 yards to the west of the first, but with no better results, for at a depth of $65\frac{1}{2}$ fathoms the same impediment was met with. After much labour and loss of time a length of 4 feet 9 inches was bored into the whin, and then no further progress was attempted.

* I am indebted to my friend Mr. Richard Howse, of Newcastle-on-Tyne, for the information given above respecting the localities in which basaltic rock is found in the North of England.

In the mean time a neighbouring firm had sunk a pit in the immediate vicinity without meeting with any trace of whin rock; and as circumstances demanded it, two shafts were commenced by Messrs. Bell Brothers 185 yards N.E. of the first bore-hole. The area within these three points amounts to something like 15 acres. When the more advanced of these pits was sunk to a depth of 67 fathoms, the men came upon the hard obstacle met with in the bore-holes; and the same thing happened in the second shaft, situate 60 yards to the west of its neighbour.

In each case fully four months of incessant labour, accompanied with considerable expense, was required before the lower surface of this hard stratum was reached, which proved to have a thickness of 19·75 feet. In all probability, however, its dimensions are subject to considerable fluctuations, for in one of these two pits the bed is 4 feet thinner on the north side of the shaft than on the south.

It was now clear that it was a bed of basalt lying in a horizontal position which was encountered, in which direction it had spread itself as one offering less resistance than that to be overcome by forcing an exit at the surface. At what point a communication exists between this interjected mass and its subterranean source we have had no means of ascertaining.

Above and below the basaltic bed, as found in the pits, there are two well-known seams of coal. In the neighbouring colliery spoken of (Littleburn) these are separated by 50·25 feet of fire-clays, shales, and sandstones, while in the present case the intervening rocks measure 103·66 feet, showing an increase of 53·41 feet. Of this only 19·75 feet is due to the whin, the remaining 33·66 feet arising from a thickening of the sandstone and other deposits.

During the entire progress of sinking specimens of the rocks were preserved, which enabled me at my leisure to examine not only the whin but also the altered character of the adjacent strata.

The change experienced by the latter has frequently formed the subject of comment; but I am not aware that much attention has been directed to any modification in the composition of the basalt itself, caused by contact with the substances through which it had penetrated.

It would of course be highly instructive if any sedimentary rock could be accepted, in respect to its constituents and their relations to each other, as a normal type of whinstone. If, for example, an aqueous rock were found in the immediate vicinity of basalt, and the compositions of both were the same, one might infer the physical difference to be due to the mere influence of igneous action.

There are, however, to be found in nature many substances which more or less resemble in constitution the matter filling whin-dykes, clay-slate being one, some specimens of which contain the following ingredients :—

Silica	54
Alumina	24
Protoxide of iron	14
Magnesia	5
Potash	3
	100

Now there would be nothing extravagant in the supposition that clay-slate of this character reduced to a state of fusion by heat might be materially altered in passing through the Mountain Limestone and Millstone-grit before reaching the surface in the county of Durham, and that it then might resemble the composition of the whin-dykes of the district. That the Hett Dyke and its parallel one derive their origin from one common source, and if changed by subsequent contact with other rocks have been altered in each case by the same cause, is apparent on referring to their respective compositions.

	Hett Dyke.	Parallel Dyke.
Silica	51·35	50·60
Alumina	17·61	17·38
Protoxide of iron	12·04	12·22
Lime	9·65	9·20
Magnesia	5·68	5·66
Potash	1·40	1·64
Soda	·56	·50
Carbonic acid	1·53	2·57
	99·82	99·77

In some measure the interior portions of the horizontal bed found in the pits resemble these two specimens in the nature of their constituent parts, and in physical appearance there is little or no difference among them. Fragments taken from the bed gave:—

Silica	51·90
Alumina	15·46
Protoxide of iron	12·87
Lime	13·80
Magnesia	4·02
Potash	1·21
Soda	·48
Carbonic acid	1·02
	100·76

As stated, the specimen just described was obtained from the interior of the mass of whin; but its exterior, both on the upper and lower surfaces, is covered with a coating 10 inches thick in the one, and

something under this in the other case. These coatings exhibit a marked similarity to each other; but a striking difference in some respects is observed to the interior of the mass they surround.

	Upper surface.	Lower surface.
Silica	43·22	40·62
Alumina	17·44	18·18
Protoxide of iron	13·03	14·00
Sulphide of iron	....	1·31
Lime	6·26	4·37
Magnesia	2·86	3·94
Potash	} 1·28	·78
Soda		·33
Carbonic acid	14·72	13·23
Water	1·46	2·36
	100·27	99·12

In colour these coatings are of a light buff, and are close in texture, in both of which particulars they differ considerably from the unchanged basalt. It would appear that any difference in composition between the interior of this horizontal bed of basalt and its outer surfaces is entirely independent of that which might be supposed to arise from mere contact with the adjoining strata. Lying above it is 8 feet of a siliceous rock known among miners in the North of England as a "white post." It was found to consist of:—

Silica	88·25
Alumina	5·69
Peroxide of iron	1·71
Lime	1·53
Magnesia	·69
Potash	·80
Soda	·21
Water	1·75
	100·63

The underside of the basalt rests upon a thin seam of coal, greatly charred by its proximity to so large a volume of matter, which must have arrived at its present position in a state of intense heat. This coal is of course deficient in volatile constituents, and contains a large percentage of ash, one half of which is carbonate of lime. Its composition is as follows:—

Carbon	56·58
Hydrogen	1·00
Oxygen	3·52
Sulphur	·18
Ash	38·65
	99·93

It will be observed that, in the first three instances quoted of the component parts of whinstone, carbonic acid is named as a constituent varying in amount from 1·02 to 2·57 per cent. of the whole. This is a little remarkable, because I confirmed by actual experiment that when protoxide of iron, lime, magnesia, potash, and soda, in the form of carbonates, are fused with silica and alumina in the proportions in which these substances are found to exist in the whin, the resulting mass does not contain, as might be expected, a trace of carbonic acid. Whether the small quantities just mentioned of this acid represent portions which under immense pressure could not escape from the lime with which it was perhaps originally combined, is a question to which no satisfactory answer can be given.

On the other hand, it is not improbable that basalt may, after or possibly during the act of cooling, be placed in circumstances where carbonic acid may be absorbed by such of its constituents as are known to form carbonates.

The following experiments were tried on a sample of the pounded whin-rock previously ascertained to contain 1·97 per cent. of carbonic acid:—

	per cent. CO_2.
a. 2·5 grammes having been exposed during four days to dry carbonic acid, then contained (being practically unchanged)	1·94
b. 2·5 grammes, after exposure during four days to moist carbonic acid, contained	2·04
c. 2·5 grammes had passed over it during five hours, at a temperature of about 500° C., a current of carbonic acid. It gave	1·10
d. 2·5 grammes treated as in *c*, the carbonic acid being moist, gave	1·70
e. 2·5 grammes, exposed at a low-red heat for five hours to a current of dry carbonic acid, contained	·29
f. 2·5 grammes treated as in *e*, but moisture was present, gave	·49

The experiment *b* in the series would indicate that when moisture is present a small quantity of carbonic acid is absorbed at ordinary temperatures. When the temperature was raised, as in *c*, *d*, *e*, and *f*, there was a perceptible loss of acid; but in *d* and *f* this loss was materially retarded by the presence of vapour of water. It is therefore conceivable that carbonic acid, if at one time entirely expelled from the fluid basalt, may have been reabsorbed, when aided by the presence of moisture and probably under enormous pressure, during the countless ages which have elapsed since it first occupied its present position.

This idea is confirmed to some extent by an examination of the com-

position of the outer coverings of the basalt, already described. These in all probability would, from their position, be more freely exposed to the action of carbonic acid than the interior of the mass of which they formed the coating. That these portions are in reality identical with the bed itself, merely altered by the rate of cooling, or by a change in composition due to absorbing carbonic acid, or by these causes combined, is easily seen when the proportions of their fixed elements are compared with those as they exist in the whin-rock of the neighbourhood.

Fixed elements.	Covering of horizontal bed.	North dyke parallel to Hett Dyke.	Interior of horizontal bed.
Silica	51·56	52·05	52·05
Alumina	20·72	17·88	15·50
Protoxide of iron	15·47	12·58	12·90
Lime	7·38	9·47	13·83
Magnesia	3·33	5·82	4·02
Potash and soda	1·54	2·20	1·70
	100·00	100·00	100·00

The chief change, therefore, in composition which the outer portions of the bed of whin have experienced is a much greater absorption of carbonic acid, amounting to nearly nine tenths of the total quantity required for converting the iron, lime, magnesia, and alkalies into carbonates.

It is perhaps a difficult task to speak with any degree of confidence on the precise nature of the causes which have given rise to certain differences in the proportions of some of the earths as exhibited in the above figures. We may, however, readily imagine that a liquefied rock, during its passage through a series of stratified beds of various kinds and of different thickness, will continue for a longer or a shorter time in contact with any given substance, according to the size of the latter, or according to the rate at which the stream of fluid matter for the time being is travelling. It is also equally easy to suppose that the exterior may solidify by contact with cooler surfaces when the basalt has assumed the composition resembling that of the crust or coverings already described. The liquid torrent continues to flow through beds in which lime preponderates, such as the mountain limestone: an additional quantity of this earth is dissolved, and the basalt, thus altered in composition, is expelled through a kind of gigantic tube formed by the cooling of the first portions of the ejected mass.

As a source of silica and alumina I would point to the composition of the rocks immediately below the thin seam of coal underlying the horizontal bed of basalt under consideration.

	Charred coaly shale.	Strong blue shale.	Grey post stone.	Fire-clay.
Thickness	0 ft. 3 in.	9 ft. 4 in.	1 ft. 3 in.	3 ft. 0 in.
Silica	39·80	52·85	67·30	61·65
Alumina	29·91	28·15	9·47	23·77
Protoxide of iron	2·60	4·62	11·81	4·64
Sulphide of iron	....	....	....	·03
Lime	·64	·68	1·01	·15
Magnesia	1·19	2·13	2·16	2·49
Potash	2·60	1·52	·61	2·24
Soda	·84	·92	·81	·21
Carbonic acid	....	....	4·53	....
Water	....	8·05	3·35	5·41
Carbon	22·24	1·30	....	....
	99·82	100·22	101·05	100·59

Having now considered the changes which the intruded igneous rock has undergone, the alterations may be examined which the presence of a vast mass of highly heated matter has effected on the adjacent sedimentary rocks.

The small amount of volatile constituents left in the thin seam of coal immediately underlying the bed of whin in the pit has been already explained. At a distance of about 17 feet below this is a second bed of coal 5 inches in thickness; but to it the intervening rocks have acted in some degree as a protection against the volatilizing power of the heated basalt lying above.

By its analysis the following results were obtained:—

Carbon	78·05
Hydrogen	3·78
Oxygen	4·32
Sulphur	4·33
Ash	10·15
	100·63

At a further depth of about 35 feet from this perhaps slightly altered coal, or 52 feet below the bed of basalt, is another nearly 5 feet thick, known as the Busty, which is one of the well-known coking seams of the country. When the coal from it is compared with that obtained from the same bed two miles away, there is perceptible difference to be observed. We may therefore infer that the emission of heat from the thick body of melted basalt lying about 50 feet above it was at the time of cooling so slow as not to have affected the composition of this coal, which loses at moderately high temperatures 35 per cent. of its weight.

Looking at the very complex nature of the composition, already referred to, of the coaly shale, strong blue shale, and grey post stone, it

seems not impossible but that some of the constituents of these strata, common to all, may owe their origin to the basalt itself, from which they may have been emitted in the form of vapour.

Although silica, alumina, and lime are regarded, and justly so, as being little affected by exposure even to a very intense heat, I am nevertheless disposed to believe that all three are susceptible of being evaporated at temperatures not unfrequently used in the arts. From iron blast-furnaces, and particularly from those in the North of England, a vast quantity of white fume or smoke is emitted, which readily condenses on a cold surface. To one of these furnaces in the county of Durham I attached an air-pump worked by steam, and by its means the whole of this fume from a given volume of gas, as it escaped from the furnace, was condensed in water.

On analysis it was found to be composed of—

Silica	14·06
Alumina and some peroxide of iron	25·70
Lime	2·30
Magnesia	trace
Chlorine	·61
Sulphuric acid	·64
Oxide of zinc	19·99
Carbonates of potash and soda	29·05
Carbonic acid	7·83
	100·18

Not being prepared to find certain of these bodies carried away in the vaporous form, the idea suggested itself that they might, in the first instance, be deoxidized in the powerfully reducing atmosphere of the hearth of the furnace, and subsequently reoxidized in its upper regions. To satisfy myself on this point, I drew a considerable volume of the gases as they exist in the hottest portion of the reducing zone and passed it through cold mercury. Had any such action as that indicated been effected, I would expect to find potassium or sodium in the mercury. No trace, however, of these metals was detected, and I therefore concluded that the furnace-vapours in question must be regarded as true sublimates. If so, and if the estimate of the quantity be correct, many thousands of tons of alumina &c. are annually evaporated on the banks of the Tees during the process of smelting iron.

If similar vapours were emitted by basalt when intensely heated, it is almost certain that, under the great pressure then prevailing at the depth of this particular bed, some portion would find its way into the adjoining strata. Lying above the basalt is some 8 or 9 feet of a rock spoken of as "white post." On referring to the analysis formerly quoted, it will be seen that this bed contains 88·25 per cent. of silica (it is there-

fore a true siliceous rock), and that its remaining 11·75 parts consist of substances all of which occur in the basalt. It is true the same may be said of the rocks underlying the stratum of whin, and that in both cases these ingredients, found in small quantities, may have been deposited with the material constituting the main body of the bed.

Believing, however, in the possibility of the basalt having been the source of all or a part of what may be designated as foreign ingredients in this white post, three specimens were submitted to examination. One was taken from the bed where it adjoins the whin, a second from its middle, and the third from its upper portion; and certainly, so far as some of these ingredients are concerned, they tend to confirm the view just expressed. This view is based on the fact that the substances in question are found in greatest quantity next the basalt, and that they gradually diminish in this respect as the distance from the basalt increases.

	Adjoining bed of whin.	Middle portion.	Upper portion.
Silica	83·17	84·31	86·22
Alumina	8·34	8·80	8·47
Peroxide of iron	....	·57	1·00
Protoxide of iron	1·32	1·03	·64
Lime	1·74	·93	·91
Magnesia	·94	·83	·22
Potash	1·02	·89	·81
Soda	·58	·52	·38
Carbonic acid	1·20	·77	·60
Water	1·50	1·70	1·20
	99·81	100·35	100·45

Before any well-established conclusions can be arrived at on the changes of composition experienced by basalt, as well as by those strata through which it has been ejected, a very extended series of observations is indispensable. At some future opportunity I may pursue this inquiry into other geological districts traversed by the 170 miles of whin-dyke described in my opening remarks; in which case, should it be the wish of the Society, I would lay before its members the results of my examination.

The analyses referred to in this communication were made at the Clarence Iron Works by Mr. Rocholl, the superintendent of the laboratory attached to the establishment.

P.S. Since writing the above, I have ascertained that the late Professor Jukes met with in the Staffordshire coal-field, and described under the name of "white rock," an altered basalt. Its composition as given is:—

Silica	38·830
Alumina	13·250
Protoxide of iron	13·830
Peroxide of iron	4·335
Lime	3·925
Magnesia	4·180
Potash	·422
Soda	·971
Carbonic acid	9·320
Water	11·010
	100·073

Although Professor Jukes expresses himself as confident of the origin of this substance, he nowhere had an opportunity of examining it in contact with basalt previous to its alteration. The presence of carbonic acid and water he ascribes to subsequent infiltration.

XV. "Results of Magnetical Observations made in Little Namaqualand during a part of the Months of April and May, 1874." By E. J. Stone, M.A., F.R.S. Received June 11, 1875.

An eclipse of the sun was to occur on April 16, 1874, which would be total throughout Little Namaqualand. I made arrangements for a visit to this country to observe the eclipse. The country is one rarely visited. I was not aware that any determinations of the magnetic elements had been made there, except a few of the variation by the Admiralty surveyors at one or two points along the coast. It appeared to me desirable that the opportunity afforded by my visit to observe the eclipse should not be lost of securing magnetical observations at several stations in Namaqualand. An application was made to the Colonial Government for some assistance. An ox-waggon was required for the transit of the magnetical equipment and of a wooden building which had been prepared to protect the instruments and the observers whilst at work. The sum asked for was sixty pounds. The request thus made was, however, refused, although with great courtesy and apparent reluctance, from a supposed difficulty in passing such a grant through Parliament. I was, however, most unwilling to abandon the idea of making these magnetical observations. When the facts of the case became known, I received offers of assistance from some gentlemen in Namaqualand, and His Excellency Sir Henry Barkly, K.C.B. &c., kindly interested himself in the matter and afforded me all the facilities in his power. I determined therefore to carry out, in a somewhat modified form, the scheme of observations which I had arranged. The wooden building was left behind. I found that good observations could be made without cover of

any kind for the instruments or the observer, although at a considerable cost in time and in the comfort of those engaged upon the work. The greatest trouble arises from the action of the wind upon the instruments, more particularly upon the dip-instrument; but by patiently awaiting opportunities the injurious effects arising from this cause can be very nearly, if not quite eliminated. I decided also, after some hesitation, to take no assistant with me. I was anxious to avoid unnecessary expense, and to obtain as great freedom for moving about the country as possible. I only absolutely required some one to enter the times from a chronometer to the nearest half-second; and I found, after a careful series of experiments upon the point, that my wife could do this without any difficulty. The probable error of these time determinations does not appear to exceed three tenths of a second; and it would be very difficult, with a skilled assistant, to obtain, under the circumstances of these observations, a much greater degree of accuracy. The instrumental equipment consisted of :—a "Dip," by Dover; a "Unifilar," variation, and intensity instrument combined, by Elliott Brothers; a five-inch Theodolite, for the determination of the latitudes, local times, and absolute azimuths of marks; aneroid barometer; thermometer; and a pocket chronometer beating half-seconds. With only two persons and this instrumental equipment, it was found possible to move freely about the country in a Cape cart, and with comparative luxury in a waggon.

I knew that a chronometer could not be trusted to carry accurate time over such a country as Namaqualand without, at least, special appliances, which were not available to me under the circumstances of the case. I arranged therefore for the determination of local time with the theodolite, and for the fixing of the absolute azimuths of the marks with the same theodolite immediately before or after the determinations of local time. The ill effects of a trip or stoppage of the chronometer was thus eliminated. This precaution was found necessary. The chronometer has stopped and then gone on again on several occasions from the dreadful shaking of the carts or waggons over the rocky roads, and has on some occasions tripped some seconds from merely being carried about in the pocket or hand. The chronometer dead-beat escapement does not appear at all well fitted for rough work of the kind, and a good lever escapement would be much preferable. The chronometer used has had a very steady rate when not moved about, and has not much changed that rate, when at rest, from the effects of the journey. The greatest practical inconvenience experienced in the use of the instruments was the occasional breakage of the suspension-threads and the loss of time in getting the new threads out of torsion. After several accidents of the kind, I used three instead of two threads of suspension; but the torsion was inconveniently large, and the loss of time in getting the threads out of torsion considerable. These three threads were only used at the Orange-River station and on the return to Port Nolloth. The time of vibration

of the magnet was found as follows :—The passage of the line of reference in the needle over the wire of the observing-telescope was noted at every tenth complete vibration passing right and passing left. If t_1, t_2, t_3, and t_4 are n observed times of these passages right, we have for the time of a semivibration

$$\tau=\frac{3}{10}\frac{1}{n+1}\left\{\frac{2}{n(n-1)}[0\times t_1+1\times t_2+2\times t_3+\ldots.+(n-1)t_n]\right.$$

$$\left.-\left[\frac{t_1+t_2+\ldots.+t_n}{n}\right]\right\},$$

and a similar expression for the time from the passages left.

The probable error of the mean of these two determinations can be shown to be

$$\frac{e}{10}\sqrt{\frac{3}{2}\frac{1}{(n-1)n(n+1)}},$$

where e is the probable error of a single time determination.

In the Namaqualand observations n was usually 6. If, therefore, $e=0^{s}\cdot3$, we have for the probable error in the times of vibration

$$0\cdot0025.$$

I think this error rather in excess than defect.

The Dip Observations consisted of a bisection of the upper and lower ends of the needle after each lift of the needle; both microscopes were read each time. There were never less than four of these independent lifts in each position; a complete dip therefore consisted of at least sixty-four independent bisections of the ends of the needle and of thirty-two independent lifts of the needle from the agate planes. A great deal of time was consumed in making one of these dips in the open air, on account of the disturbances of the instrument by the wind. In the zenith-distance observations of the sun for local time, and azimuth observations for the determination of the absolute azimuths of the marks, both limbs of the sun in reversed positions of the theodolite were always taken. The differences between the azimuths of these marks, usually two, and the azimuthal reading for the magnetic meridian were taken with the Elliott instrument. The means of the results obtained from the variation-needle, which allows of reversed suspension, were alone used; but the reading for the magnetic axis of the vibration-magnet, which is well adjusted, was usually taken as a check. To save time the deflection observations were only made at the distance of one foot, except for the Port Nolloth station. The small correction to the results thus found, usually determined by a second set of deflections at 1·3 foot, has been obtained from the Port Nolloth observations and other determinations at the observatory. The longitudes of the stations are only very rough approximations. No attempt was made to fix them with any greater accuracy than

that required for the necessary interpolations of the sun's declination. I may perhaps be permitted to mention that the observations whose results are contained in the present paper could hardly have been made had it not been for the assistance afforded me by E. J. Carson, Esq., Manager, and R. T. Hall, Esq., C.E., Engineer, of the Cape Copper Company. My thanks are due to them for a thoughtful kindness which offered every facility for my work, and yet rendered a working trip into a somewhat wild country one of great enjoyment.

Port Nolloth.

Station not far from the Cemetery, Sandy Velt.
Longitude $1^h\ 7^m\ 28^s$. South latitude 29° 15′ 30″.

Dip Observations. Needle A_2 B, Dover.

			°	′	″
1874, April 10, 11^h.	A_2 South	....	53	19	30
	A_2 North	....	53	26	3
	Dip	.........=	53	22	46

The wind was very high at times during these observations; but the results appeared satisfactory.

1874, April 11, 10^h. Temperature 61°·2.

Deflections produced by the vibrating-magnet:—

$$1\cdot0\ \text{foot}\left\{\begin{array}{rrr} 10^\circ & 56' & 43'' \\ 10 & 57 & 7 \\ 10 & 55 & 52 \\ 10 & 57 & 3 \end{array}\right\} u=10^\circ\ 56'\ 41''.$$

$$1\cdot3\ \text{foot}\left\{\begin{array}{rrr} 4 & 56 & 3 \\ 4 & 59 & 42 \\ 4 & 56 & 33 \\ 5 & 1 & 7 \end{array}\right\} u'=4^\circ\ 58'\ 21''.$$

April 11, $12^h\ 30^m$. Temperature 59°. $90p=2'$=torsion. $\tau_1=5^s{\cdot}05$, $\tau_2=5^s{\cdot}05$.

Wind so high that the observations had to be abandoned. These determinations have not been used.

Variation Observations.

1874, April 12, 10^h. Temperature 75°. $90p=2'\ 10''$. Correction for torsion insensible.

Readings for variation-magnet:—

	°	′	″
Suspension direct	265	55	30
,, reversed	264	40	22
Azimuthal reading for magnetical meridian	265	17	56
Azimuthal reading for magnetical meridian with vibration-magnet........................	265	18	5
Reading for southern mark	288	8	15
,, northern mark	261	20	35

The absolute azimuths of these marks determined on April 10 were as follows:—

	°	′	″
Azimuth of northern mark	147	8	22
„ southern mark	6	4	33
Variation from northern mark	28	54	17
„ southern mark	28	54	52
Variation, April 12, 10^h	28	54	35

Vibration Observations.

April 12, 11^h 20^s. Temperature 72°. $90p=2' 10''$. $\tau_1=5^s{\cdot}0543$, $\tau_2=5^s{\cdot}0529$, $\tau=5^s{\cdot}0536$.

The changes of temperature were considerable; at times a cold damp mist passed over from the sea.

The latitude was determined on April 12, near noon, and found to be 29° 15′ 30″.

On my return to Port Nolloth I was detained six days waiting for the steamer, which lay outside the bar, but could not cross on account of rough weather. During the greater part of this time a dense damp mist prevailed that rendered observations impossible. I was, however, anxious to repeat, at least, the determination of the variation; and this was done on May 3, 3^h. The station was one rather nearer the sea than that first chosen.

Temperature 70°·2. $90p=10' 17''$. Correction $-0' 55''$.

Azimuthal reading for magnetical meridian, May 3, 3^h:—

	°	′	″
Suspension direct	319	17	24
„ reversed	318	51	48

	°	′	″
Reading =	319	4	36
Torsion correction =		0	55
	319	3	41
Reading for mark	320	27	2

Excess of reading for mark .... 1° 23′ 21″

The azimuth of the mark was found as follows:—

	°	′	″
1874, May 3	152	26	49
May 5	152	26	41

The variation on May 3, 3^h=28° 56′ 36″. The results for Port Nolloth are therefore as follows:—

V=Variation	=28° 55′ 36″	mean of results for April 12 and May 3.
X=Horizontal force	= 4·4464	
m=Magnetic moment	= 0·4264	
D=Dip	=53° 22′ 46″	
F=Total force	= 7·4540	

Klipfontein Station.

Approximate longitude............ $1^h\ 10^m\ 45^s$
Approximate south latitude........ 29° 14′ 15″

This station was on a mountain-range about 3000 feet above the level of the sea. It was near Mr. Hall's cottage, but sufficiently removed from it to avoid all danger of disturbances from the iron in or about the buildings.

Dip Observations. Needle A_2 B, Dover.

		°	′	″
1874, April 14, 10^h.	A_2 South	53	15	35
	A_2 North	53	28	17
	Dip .... =	53	21	56

Variation Experiments.

April 15, $9^h\ 30^m$. Temperature 77°·2. Torsion correction insensible. Variation-magnet:—

	°	′	″
Azimuthal reading, suspension direct	90	52	42
,, ,, ,, reversed	92	8	20
Azimuthal reading for magnetic meridian........	91	30	31
Azimuthal reading for magnetic axis of vibration-magnet	91	28	0
Azimuthal reading for mark α	107	11	48
Azimuthal reading for mark β	352	49	17
Absolute azimuth of mark α	224	4	38
Absolute azimuth of mark β	109	42	7

Variation, 28° 23′ 21″ from α.
,, 28 23 21 from β.

The determinations of the absolute azimuths of the marks are as follows:—

			°	′	″
April 15, $6^h\ 54^m$ A.M.	Azimuth of	α =	224	5	42
,, 15, 4 49 P.M.	,,		224	3	49
,, 16, 7 22 A.M.	,,		224	5	10
,, 15, 4 49 P.M.	,,	β =	109	41	9
,, 16, 7 22 A.M.	,,		109	43	4

No direct determination of the latitude could be made under favourable circumstances at noon or equally distant from and near noon, on account of the only stand being occupied at these hours for the magnetical observations. It would appear, from the small discordance between the evening and morning determinations of the azimuth of the marks, that the adopted latitude is slightly in error. The mean of the evening and morning determinations should, however, be sensibly accurate, and has been adopted.

Vibrations.

Three sets were taken; the first two were observed at unequal intervals, and were made somewhat under difficulties. Some ostriches bore down upon the instrument and had to be continually driven from it. The mean of the first two sets, however, agrees very closely with the third.

From the first two sets $\tau = 5^{s}\cdot0608$.

The last set gave :—

1874, April 15, $11^{h}\ 6^{m}$. Temperature $80^{\circ}\cdot2$. $90p = 3'\ 53''$. $\tau_1 = 5^{s}\cdot0600$, $\tau_2 = 5^{s}\cdot0603$, or $\tau = 5^{s}\cdot0602$.

The value $\tau = 5^{s}\cdot0606$ has been adopted.

Deflections.

April 15, 3^{h}. Temperature $87^{\circ}\cdot2$.

$$\text{Distance } 1\cdot0 \text{ foot}\left\{\begin{array}{rrr} \overset{\circ}{10} & \overset{\prime}{48} & \overset{\prime\prime}{35} \\ 10 & 52 & 27 \\ 10 & 55 & 28 \\ 10 & 54 & 40 \end{array}\right\} u = 10^{\circ}\ 52'\ 48''.$$

Hence for the Klipfontein station we have :—

Variation..........	V=	28° 23′ 21″
Horizontal force	X=	4·4343
Magnetic moment ..	m=	0·4279
Dip	D=	53° 21′ 56″
Total force	F=	7·4312

The total eclipse of the sun was observed from this station on April 16. The sky was perfectly clear from clouds during the whole day.

Ookiep Station.

Approximate longitude	$1^{h}\ 11^{m}\ 33^{s}$
Approximate south latitude......	29° 36′ 15″

This station was 3059 feet above the level of the sea. This height is derived from Mr. Hall's levelling. The instruments were placed as near Mr. Carson's house as would insure freedom from any disturbing effect of the iron about the house. Ookiep is surrounded by mountains, which impeded very early or late observations of the sun from the station chosen. It is the chief mining station, at present, of the Cape Copper-Mining Company.

Dip Observations.

			°	′	″
1874, April 18, 11^{h}.	A_2 South	....	53	9	58
	A_2 North	....	53	34	44
	Dip................ =		53	22	21

Deflections.

April 20, 3^h. Temperature 82°.

$$\text{Distance 1·0 foot}\left\{\begin{array}{rrr} {}^\circ & {}' & {}'' \\ 10 & 52 & 20 \\ 10 & 50 & 50 \\ 10 & 52 & 46 \\ 10 & 56 & 29 \end{array}\right\} u=10^\circ\ 53'\ 6''.$$

Vibrations.

April 20, $4^h\ 21^m$ P.M. Temperature 67°. $90p=2'\ 50''$. $\tau_1=5^s{\cdot}0593$, $\tau_2=5^s{\cdot}0628$, $\tau=5^s{\cdot}0611$; from which $X=4{\cdot}4252$, $m=0{\cdot}4262$.

Variation.

April 20, 5^h. Temperature 67°. $90p=2'\ 50''$.

	°	′	″
Readings for the variation-magnet suspension.. {	229	3	40
	227	40	30
Reading for magnetic meridian	228	22	5
Reading for magnetic meridian, vibration-magnet .	228	23	12
Reading for chimney (2)	271	40	1
Excess of reading for chimney (2)..............	43	17	56

An attempt was made to obtain another determination of the variation on April 22, but the thread broke during the observations.

April 23, 10^h. Temperature 82°.

	°	′	″
Readings for magnetic meridian suspension.... {	196	14	33
	194	50	50
Reading for magnetic meridian	195	32	42
,, Kokerboom	220	16	8
,, chimney (1)	237	19	36
,, chimney (2)	238	46	49

Excess of reading for Kokerboom	24°	43′	26″
,, ,, chimney (1)....	41	46	54
,, ,, chimney (2)....	43	14	7

The latitude was determined April 20, near noon.

Absolute azimuths of marks:—

		°	′	″
April 20.	Azimuth of Kokerboom	183	37	6
April 22.	,, ,, ...	183	35	36
	Azimuth of Kokerboom	183	36	21
April 20.	Chimney (2)....................	165	5	46
April 22.	Chimney (1)..........	166	31	20

The two determinations of the azimuth of Kokerboom do not agree so closely as could be wished. I cannot find any thing wrong in the

reductions; and I fear that on one of the days (April 20 or 22) the theodolite must have been slightly disturbed in changing the sun-shade after the observations of the sun and before the observations of the marks. I am not, however, aware of any reason for assuming that such a disturbance actually did take place, and I have taken the mean of the two determinations as the true result.

		°	′	″	
Variation on April 20	....	165	5	46	
		43	17	56	
		208	23	42	or 28° 23′ 42″
Variation on April 22.	Kokerboom	183	36	21	
		24	43	26	
		208	19	47	or 28° 19′ 47″
	Chimney (2) ..	165	5	46	
		43	14	7	
		208	19	53	or 28° 19′ 53″
	Chimney (1) ..	166	31	20	
		41	46	54	
		208	18	14	or 28° 18′ 14″

Variation, April 22 .... 28° 19′ 18″

The results for the Ookiep station are:—

		°	′	″
Variation	$V=$	28	21	30
Dip	$D=$	53	22	21
Horizontal force	$X=$	4·4252		
Magnetic moment ..	$m=$	0·4262		
Total force	$F=$	7·4171		

Orange-River Station.

Approximate longitude .... $1^h\ 12^m\ 56^s$

Approximate south latitude .... 28° 53′ 7″

The observations were made near New Raman's, Nisbetbath, or Schuyte drift. These three names are given to fix the particular drift to which reference is made. It was a narrow gorge, surrounded by mountains of some considerable height, some rising to 3000 and 4000 feet. The height of the station above the sea appeared, from barometrical determinations, to be about 780 feet. A mountain-pass, of about 750 feet, separated the river from the Bushman flats. These flats, in this neighbourhood,

appeared, from similar barometrical determinations, to be from 1500 to 1700 feet above the sea-level. The position was not altogether a favourable one for magnetical observations. I had but little choice of stations. I fear some little local magnetic disturbances in the observations made here, such that the elements might have been found to have differed somewhat considerably with a comparatively small shift in geographical position. I should therefore have been glad to have supplemented these observations with a set on the Bushman flats, the bed of a recent sea with numerous islands (kopies); but this could not have been done without proper arrangements for the supply of water.

Dip Observations.

		°	′	″
April 26, 9^h.	A_2 South	53	39	18
	A_2 North	54	0	14

$$\text{Dip} = 53° \; 49' \; 46'',$$

a larger dip than that obtained at any other station by about 2′.

The latitude was determined near noon.

Variation.

April 26, 1^h.

	°	′	″
Azimuthal reading of mark	266	33	45
Variation-magnet	351	26	45
	350	28	33
Reading for magnetical meridian	350	57	39
Reading with vibration-magnet	350	58	50
Azimuthal reading of mark repeated	266	34	0
Excess reading for the magnetic meridian	=84	23	46

Vibrations.

April 26, 2^h. Temperature 80°. $90p = 6' \; 45''$.

Three threads were used for suspension at this station, but the torsion was carefully got rid of.

$$\tau_1 = 5^s{\cdot}0988, \; \tau_2 = 5^s{\cdot}0920, \; \tau = 5^s{\cdot}0954.$$

Deflections.

April 27, 10^h. Temperature 79°·5.

$$\text{Distance 1·0 foot} \left\{ \begin{array}{rrr} \overset{\circ}{10} & \overset{\prime}{59} & \overset{\prime\prime}{45} \\ 11 & 3 & 50 \\ 11 & 4 & 10 \\ 11 & 0 & 40 \end{array} \right\} u = 11° \; 2' \; 6''.$$

Hence $X = 4{\cdot}3798$, $m = 0{\cdot}4271$.

The results for this station are:—

Variation	..........V=	28° 27′ 24″
Dip	................D=	53 49 46
Horizontal force	....X=	4·3798
Magnetic moment	....m=	0·4271
Total force	..........F=	7·4210

The numerous observations for time at the different stations have not been given, as of no interest.

I arrived in Namaqualand on April 9, by the Union steamship 'Namaqua,' Captain Barker, reached Port Nolloth, on my return, on the evening of Wednesday, April 29, but did not sail until Wednesday, May 6, reaching the observatory on Saturday, May 9.

XVI. "On the Proportions of the several Lobes of the Cerebrum in Man and in certain of the higher Vertebrata, and on an attempt to explain some of the Asymmetry of the Cerebral Convolutions in Man." By JOHN MARSHALL, F.R.S., F.R.C.S.E., Professor of Surgery, University College, London, &c. Received June 17, 1875.

1. I desire to communicate to the Royal Society the fact that I have, by severing the cerebral hemispheres in certain definite directions in Man, and also in some of the higher Vertebrata, and by then weighing the separated portions, not only arrived at some interesting and important results as to the relative size of those portions in different animals and in Man, but I am enabled to state that this method, applied to the brains of individuals of different race, sex, age, education, and occupation, seems likely to furnish a means of investigating individual peculiarities in the human cerebrum.

I propose shortly to communicate my results to the Society.

2. I have likewise made numerous observations on the convolutions of the human brain with the view of explaining their symmetry in certain regions, and their asymmetry in others. In endeavouring to trace more particularly the causes of the asymmetry of the convolutions which prevails in Man, I have been led to believe that some, at least, of this is due to the right-handedness of Man.

I find, on studying a large number of human cerebra, that there are stronger evidences of *essential* asymmetry, as distinguished from what I would term *non-essential* asymmetry, in the immediate neighbourhood of the left fissure of Rolando, and next to this part in the right parietal lobule.

There are certain secondary essential asymmetrical conditions which may be pointed out, and besides this many non-essential and very variable ones.

Evidence can be given in support of these propositions from the examination of fœtal brains and the brains of idiots, the former of which exhibit a remarkable, *early*, and special tendency to deviations in symmetry in the neighbourhood of the left fissure of Rolando.

I regard this as merely a preliminary notice of a future communication.

XVII. "On the Influence of Stature on the Weight of the Encephalon and its parts in Man." By JOHN MARSHALL, F.R.S., F.R.C.S.E., Professor of Surgery, University College, London, &c. Received June 17, 1875.

Being anxious, for a certain special purpose, to determine the influence of stature on the weight of the encephalon and its parts in Man, I have, with the consent and ready assistance of Dr. Robert Boyd, further analyzed the MS. records of the numerous data accumulated by him, from which he framed his tables published in the Philosophical Transactions for 1861.

1. I find, first, that, as might be expected, an increase of stature is accompanied by an increase in the absolute weight of the encephalon or entire brain in both sexes.

Taking both sexes together, the total increase, with a mean range of 11 inches between the highest and lowest group of individuals examined, is about $6\frac{3}{4}$ oz. av.; in the male series, with a mean range of 7 inches, it is $2\frac{3}{4}$ oz.; in the female series, with a mean range of 6 inches, it is $1\frac{1}{4}$ oz.

Of this increase in absolute weight, the cerebrum, in both sexes, necessarily takes a larger share than the cerebellum; but having regard to the relative weights of those two organs, viz. as about 8 to 1, their relative increase is as about 11 to 1; so that the cerebrum increases absolutely more than the cerebellum.

The pons with the medulla follows the rate of increase of the cerebellum.

2. But, secondly, I find that, notwithstanding this absolute increase in the weight of the encephalon and its parts in obedience to an increase of the stature, the increase itself is not *pari passu* with the stature; on the contrary, there is a gradual and progressive relative diminution in the proportion of encephalic substance to the stature as this latter itself increases.

This is equally true if we take the range of stature in both sexes together or in either sex separately.

Hence shorter persons of either sex have, proportionately to their height, a larger amount of brain than taller ones. Nevertheless the proportion is larger in the male than in the female, not only generally, but even at corresponding heights—as, for example, in short men as compared with tall women. This is interesting, as evidence that the well-known

sexual difference in the weight of the male brain overrides the influence of stature, or subsists in spite of his greater stature, which has a tendency to diminish his proportionate amount of brain.

Further comparisons show that the stature ratio, as it may be called, diminishes with the height less markedly and less constantly in the case of the cerebrum than in the case of the cerebellum, which latter organ, therefore, obeys the influence of stature more exactly and implicitly, so far as regards its relative proportion to the body.

3. It becomes evident, and may be shown, that all estimates of other influences regulating the brain-weights in Man, whether these be sex, age, occupation, education, or disease, are liable to error unless the influence of stature be first eliminated. Instances of this statement are easy to find.

4. It may in this mode be demonstrated what is the true influence of sex, age, disease, and other modifying causes.

5. Lastly, in this way alone can we arrive, as it seems to me, at a correct appreciation of that residual cause of peculiarity or deviation in the weight of the encephalon and its parts, especially of the cerebrum, which may be called its *proper weight=variation*, as an independent or quasi-independent organ.

6. This residual variation, it may be thus shown, is far larger than any other, a fact obviously of the highest interest and importance.

In support of these propositions, and others of greater detail, I propose to make a lengthened communication hereafter.

XVIII. "On a General Method of producing exact Rectilinear Motion by Linkwork." By A. B. Kempe, B.A., of the Inner Temple, late Scholar of Trinity College, Cambridge. Communicated by J. J. Sylvester, F.R.S. Received June 4, 1875.

Since the invention by James Watt, in 1784, of the 3-bar linkwork known as "Watt's Parallel Motion," which gives an approximate rectilinear motion, many attempts have been made to obtain a more perfect solution of the problem how to obtain accurate rectilinear motion by means of linkwork. Professor Tchebicheff succeeded in obtaining a 3-bar linkwork giving a much closer approximation to a true result; but in his case, as in that of others, the solution is only approximate, and it may be, in fact, shown that with 3 bars an accurate result cannot be obtained. It was not until 1864 that the problem was solved; in that year M. Peaucellier made his memorable discovery of an accurate 7-bar solution; and in 1874, when the subject was brought prominently forward in England by Professor Sylvester, Mr. Hart, in a paper read before the British Association, gave a solution by means of 5 bars. Both these linkworks, as is now well known, depended upon the inversion of a circle with respect to a point on its circumference.

M. Peaucellier's apparatus is shown in fig. 10. P O, O K, K D, D P are four equal bars jointed together at their extremities; P B, K B are two bars also equal, but unequal to the four others; they are jointed to the others at P and K and to a fixed pivot at B. It is then easily seen that, however this linkage* is deformed, B, O, D remain in a straight line, and the product B O, B D is constant. Thus if D be made, by means of the bar A D jointed to the fixed point A, whose distance from B equals A D, to describe a circle through B, the point O will describe the inverse of the circle—that is, the straight line O L perpendicular to B A.

Mr. Hart's apparatus is shown in fig. 15. For the six bars B P, B K, O P, P D, D K, K O of M. Peaucellier in fig. 10 he substitutes the four bars

$$BC = B'C', \quad CD = C'D',$$

and takes three points, P, O, V, on a line parallel to CC'; these points, however the linkage be deformed, lie in a straight line, and the product P V, P O is always constant.

Thus V being made, by the bar V U equal to P U and pivoted at U, to describe a circle passing through the fixed point P, as in the case of M. Peaucellier's linkwork, O describes the straight line O L perpendicular to P U.

A passage in a lecture on M. Peaucellier's discovery delivered by Professor Sylvester at the Royal Institution, in which he pointed out that there might be other solutions, led me to investigate the subject further; and I succeeded in obtaining certain 7-bar linkworks producing rectilinear motion, depending on two bars being made to make equal variable angles in opposite directions with a third bar. These results were described in a paper published in the 'Messenger of Mathematics' of December 1874; they are shown in figs. 6, 12, 13, 14 of this paper, and will be further referred to.

Further investigation led me to the discovery that all these linkworks depended for their production of straight lines on an exceedingly simple and obvious property of any quadrilateral whose sides are of constant length. The observation of this property at once led to the discovery of a large number of new 7-bar linkworks, of which M. Peaucellier's, Mr. Hart's, and those previously discovered by myself proved to be particular cases, the inversion property of the two former being, so to say, accidental.

It is the object of this paper to point out this property, and how it may be taken advantage of in the construction of a number of 7-bar straight-line-producing linkworks.

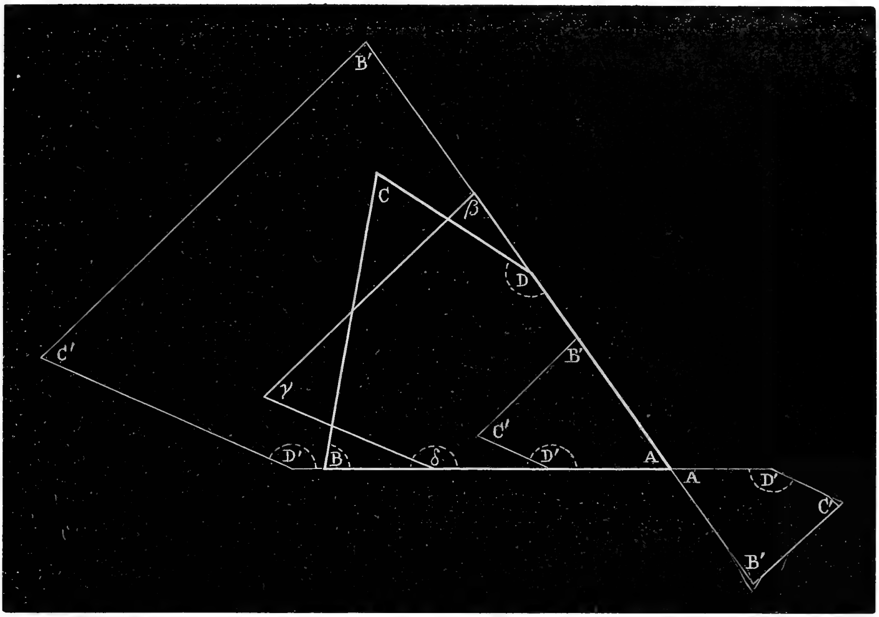
* Professor Sylvester has employed this term to mean a network composed of an even number of bars. When one bar is fixed, so that its joints become fixed pivots, the system is termed a linkwork.

The property alluded to is this :—

The cosines of the opposite angles of any quadrilateral whose sides are of constant length, but whose angles are variable, bear a linear relation to each other.

§ 1. In fig. 1, A B C D is any quadrilateral of which the sides A B, B C, C D, D A are of the lengths a, b, c, d respectively.

Fig. 1.



Then it is clear that

$$a^2+b^2-2ab\cos B=c^2+d^2-2cd\cos D. \qquad (1)$$

That is, there is a linear relation of the most general character between the cosines of the variable angles B and D.

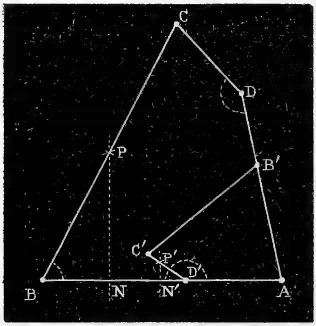
Before, however, this property can be taken advantage of something more is required; the angles whose cosines bear a linear relation to each other are the opposite angles of a closed quadrilateral; and for our purpose it is necessary that they should be the angles at the base of an open trilateral—*i. e.*, to employ the language of linkwork, the angles made with a third bar by two bars which are jointed to it. To effect this transformation let the second quadrilateral A $\beta\,\gamma\,\delta$ be constructed equal in every respect to A B C D, and having its sides δ A, β A collinear with the sides B A, D A of A B C D, but placed in a reverse position so as to be the image of A B C D. This new quadrilateral may be termed the "conjugate image" of A B C D, the whole figure forming what may be termed a "self-conjugate sextilateral."

It is clear that the angle δ is equal to the angle D; thus we have the sides B C, $\delta\gamma$ of the open trilateral C B $\delta\gamma$ making angles with A B whose cosines bear a linear relation to each other however the figure be deformed.

Since, however, the relation is an angle relation, it is unnecessary that the conjugate image should be equal to the original quadrilateral; for if the figure A B′ C′ D′ be constructed similar to A $\beta\gamma\delta$ the angle D′ is clearly equal to the angle δ, and we have the sides C B, C′ D′ making angles with A B whose cosines bear a linear relation to each other. This makes our results more general; and we are moreover able to make the points D and B′, or the points D′ and B, coincide if necessary. This more general form of figure, consisting of two quadrilaterals, one of which is the enlarged or reduced positive or negative image of the other, may still be appropriately termed a "self-conjugate sextilateral," the quadrilaterals being still called the one the "self-conjugate image" of the other.

§ 2. Now let the linkage in fig. 2 be constructed,

Fig. 2.



in which

$AB = a$,	$AB' = ka$,
$BC = b$,	$B'C' = kb$,
$CD = c$,	$C'D' = kc$,
$DA = d$,	$D'A = kd$,

k being positive or negative, and greater, equal to, or less than unity, so that the linkage forms a self-conjugate sextilateral, the quadrilaterals A B C D, A B′ C′ D′ being self-conjugate images the one of the other.

Now take any point P on B C, and let B P $= \lambda$, and take a point P′ on D′ C′ such that D′ P′ $= \lambda \frac{cd}{ab}$. Draw P N and P′ N′ perpendicular to A B. Then

$$BN = \lambda \cos B,$$

$$D'N' = \lambda \frac{cd}{ab} \cos D' = \lambda \frac{cd}{ab} \cos D,$$

and

$$B D' = a - kd.$$

Thus $N N' = B D' - B N + D' N'$*

$$= (a - kd) - \frac{\lambda}{2ab}\{2\,ab \cos B - 2\,cd \cos D\},$$

by (1)

$$= (a - kd) - \frac{\lambda}{2ab}\{(a^2 + b^2) - (c^2 + d^2)\}, \quad\ldots\ldots\ldots\ldots\ldots\ldots (2)$$

a constant. On the other hand, $P N - P' N'$ is in general variable.

The linkage in fig. 2, which will assume innumerable forms by giving different values to a, b, c, d, and k, is the fundamental linkage upon which the various linkworks here discussed depend. As the same lettering will be preserved throughout the diagrams, the fundamental linkage may be at once recognized in each figure showing its various adaptations.

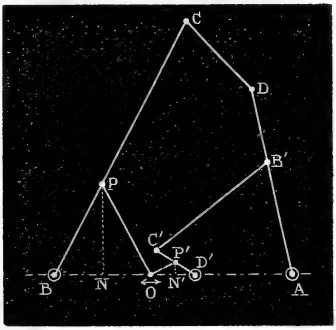
For clearness the bars are denoted by thick lines, the joints by round spots; when a bar becomes fixed so that its joints are fixed pivots, the bar is denoted by a broken line and the pivots by circles round the spots. When points in general separate are made coincident, the letters denoting all the coincident points are bracketed together. It is found convenient to collect the different linkworks into four groups, a separate section, numbered to correspond with the figure, being devoted to each separate linkwork described.

I. § 3. Take

$$\lambda = \frac{(a - kd)ab}{(a^2 + b^2) - (c^2 + d^2)}$$

so that $N N' = \frac{B D'}{2}$.

Fig. 3.



Then if the bar $A B$ be fixed and two bars $P O$, $P' O$ be added,

$$P O = P B, \quad P' O = P' D';$$

O clearly lies on $A B$ however the linkwork be deformed, and its locus is therefore the straight line $A B$.

* In the figure $D' N'$ is negative.

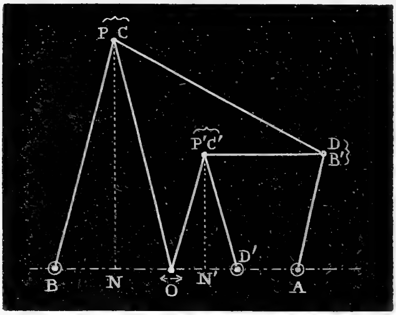
§ 4. If in this last linkwork we make

$$b=c,\ k=\frac{d}{a},\ \text{then } \lambda=b,$$

and

P coincides with C,
P′ ,, ,, C′,
B′ ,, ,, D.

Fig. 4.



The linkwork then assumes the form given in fig. 4.

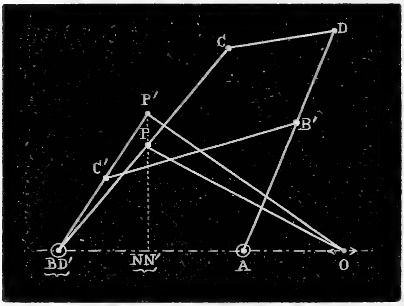
§ 5. Again, in the linkwork of § 3, if

$$k=\frac{a}{d} \text{ and } a^2+b^2=c^2+d^2,$$

then $\lambda=\frac{0}{0}$ and is indeterminate. P may then be taken anywhere on B C; and since N N′=0, P′ lies vertically above P. Also P O and P′ O may be taken of any lengths so long as

$$P'O^2-PO^2=P'B^2-PB^2.$$

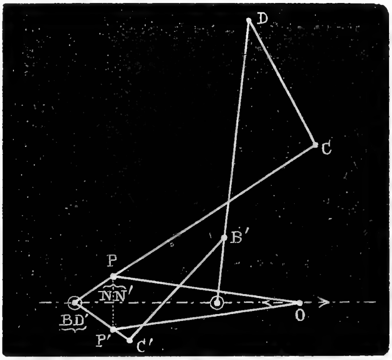
Fig. 5.



§ 6. Here we have a special case of the linkwork in the last section in which $a=-c$, $b=d$. In this case C B and C′ D′ are equally inclined

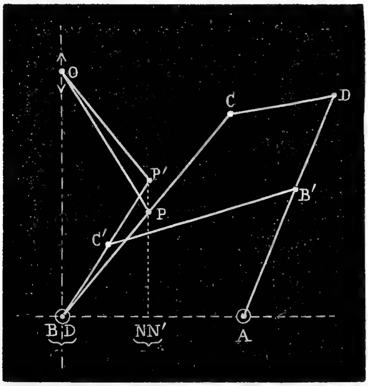
to B A in opposite directions; and the linkwork is one of those given by me in the 'Messenger.'

Fig. 6.



§ 7. This case does not strictly come under the same head as those coming before, but is an exceptional one.

Fig. 7.



Since P and P′ in § 5 lie vertically the one over the other, it is clear that if the links P O = P′ B, P′ O = P B be added, O lies on the straight line O B perpendicular to A B, and thus O B is the locus of O.

II. § 8. Make $k = \frac{a}{d}$ in the fundamental linkage, so that B and D′ coincide.

Fix A B, add the bars R P O, R P′ O′, making

$$R P = P\ O = P' D',$$
$$R P' = P' O' = P B,$$

then

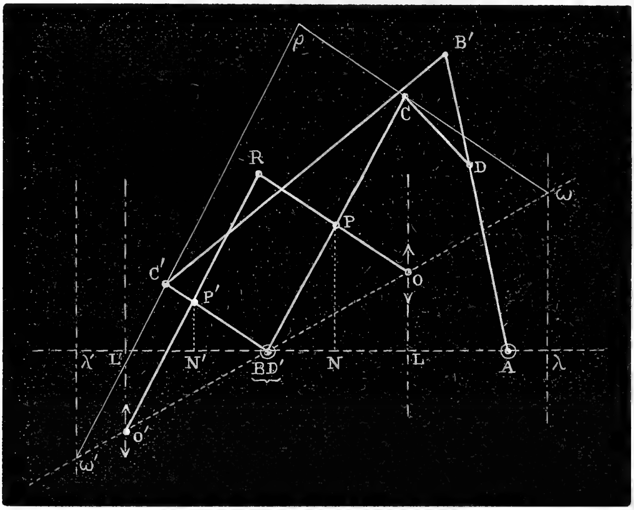
$$R\,P\,O \text{ is parallel to } P'\,D',$$
$$R\,P'\,O' \quad \text{,,} \quad \text{,,} \quad P\,B.$$

Thus, if $O\,L$, $O'\,L'$ be drawn perpendicular to $A\,B$,

$$N\,L = N'\,D',$$
$$N'\,L' = B\,N.$$

Therefore $B\,L = B'\,L' = N\,N'$, a constant.

Fig. 8.



Thus the loci of O and O′ are two parallel straight lines perpendicular to A B.

The two added bars may clearly be replaced by the bars $\varrho C \omega$, $\varrho C' \omega'$ parallel to them; and the points ω, ω', where these bars cut the line $O'\,B\,O$, will move in straight lines $\omega\lambda$, $\omega'\lambda$ perpendicular to $A\,B$.

III. § 9. Fix A B.

Make $k = \dfrac{d}{a}$, $\lambda = b$.

Then P coincides with C,

P′ ,, ,, C′,

D ,, ,, B′.

Replace $C'\,D'$, $C'\,B'$ by the new bars

$$B'\,K = C'\,D',\ \ D'\,K = C'\,B',$$

so that $B'\,K$ is parallel to $C'\,D$.

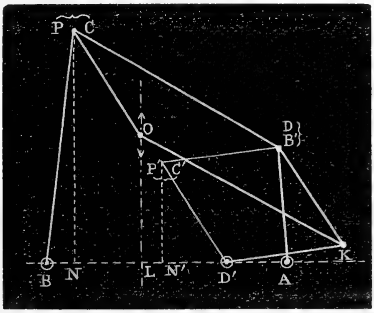
Add the two bars

$$C\,O = B'\,K,\ \ K\,O = B'\,C,$$

so that $P\,O$ is parallel and equal to $D\,K$, and therefore to $C'\,D'$.

Draw O L perpendicular to A B.

Fig. 9.



Then N L=N′ D′.

Therefore B L=B N+N L=N N′, a constant.

Thus the locus of O is the straight line O L perpendicular to A B.

§ 10. Now in the last linkwork make

$$a=d.$$

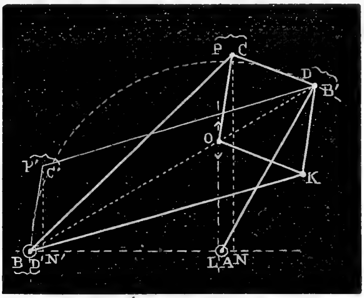
Then B coincides with D′,

D′ K=C B,

P O =O K=K D=D C,

and the linkwork becomes that of M. Peaucellier.

Fig. 10.

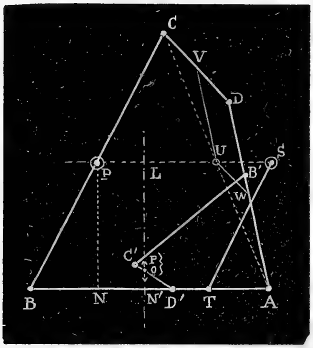


IV. § 11. Taking the fundamental linkage in its most general form, fix the point P on a pivot. Now if the bar A B be made to remain always parallel to the fixed line P S, since N N′ is constant, P′ will move on the straight line P′ L perpendicular to P S.

The parallelism of A B is effected most obviously by adding the bar S T equal to P B, P S being equal to B T. Other methods may, however, be employed; for if C A be joined cutting P S in U, U is a fixed point; and if U V be drawn parallel to C D, U V is constant and V is a fixed point on C D. So if U W be drawn paralled to C D, U W is constant and W

is a fixed point on A D. Thus the bar S T may be replaced by either of the bars U V or U W.

Fig. 11.



§ 12. In the case in which the bar S T is employed in the last section, make

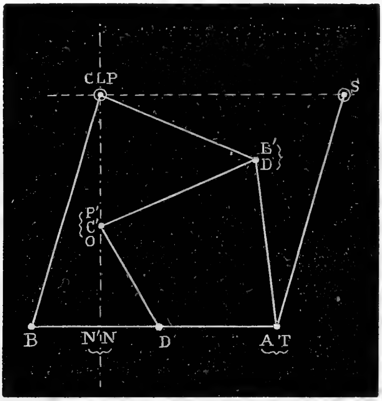
$$a=b,\ c=d,\ k=\frac{d}{a},\ \lambda=b,$$

and make T coincide with A. Then

C, L, P coincide,
P′, C′, O ,,
A, T ,,
B′, D ,,
N, N′ ,,

and the bars C D, D O are equally inclined to C S in opposite directions.

Fig. 12.



This linkwork is one of those given by me in the 'Messenger.'

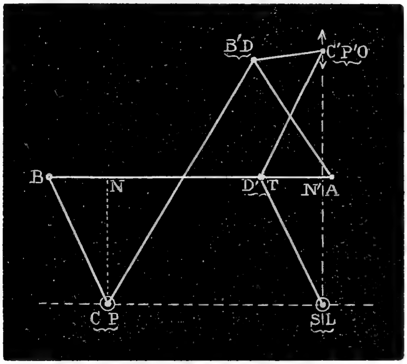
§ 13. In the linkwork of § 11 make

$a=c,\ b=-d,\ \lambda=b,\ =k\dfrac{d}{a}$, and make D′ and T coincide. Then

S, L coincide,
C, P ,,
D′, T ,,
C′, P′ O ,,
B′, D, ,,

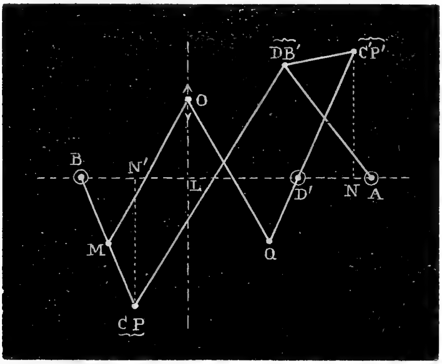
and the bars B C, D′ C′ make equal angles with A B in opposite directions; and the linkwork is one of those given by me in the 'Messenger.'

Fig. 13.



§ 14. The peculiar form of the fundamental linkwork employed in the last case may easily be seen to be really the same as was used in § 5. From the property of the equal inclination of the bars B C, D′ C′ to B A, another form of linkwork may be obtained which does not, strictly speaking, come under this group, but is an exceptional one.

Fig. 14.

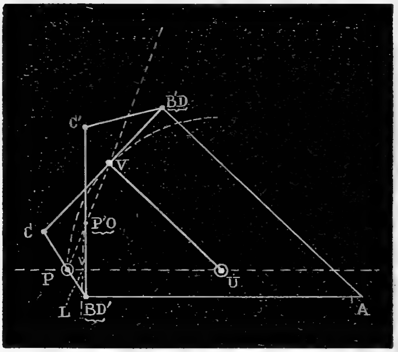


C′ D′ is produced to any point Q and a point M is taken on B C such that B M=D′ Q. A B is fixed. The equal bars M O, Q O are added. Then O clearly moves on the straight O L perpendicular to A B, B L being equal to L D′. This linkwork was given by me in the 'Messenger.'

§ 15. In the linkwork of § 11 replace the bar S T by the bar U V. Make

$$a=d, \ k=\frac{d}{a}=1.$$

Fig. 15.



Then it will be seen that

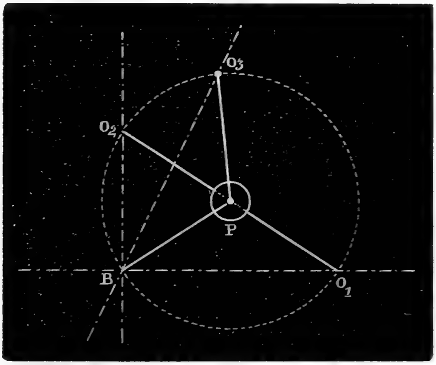
B coincides with D′,
B′ „ „ D,

and the bars A D, A B become superfluous and may be removed. There then remains Mr. Hart's 5-bar linkwork. This is the only 5-bar linkwork giving rectilinear motion that has as yet been obtained.

§ 16. In many of the previous cases the point O, which moves in a straight line, will be found to be connected to a fixed point through which the straight line passes by two equal bars. Whenever this occurs the motion of the bar containing the moving point is of the sort described by Professor Sylvester as "tram motion." That is, its motion is that of a bar sliding between two fixed rectilinear trammels, or, to present this motion in its fullest generality, the motion of a plane in which a whole series of points lying on a circle move each of them in straight lines passing through a fixed point. Turning to fig. 16, if B P, P O_1 be equal bars and O_1 moves in the straight line O_1 B passing through B, then if O_1 P be produced to O_2 so that P O_2=P O_1, O_2 obviously moves in the straight line B O_2 perpendicular to O_1 B, so that $O_1 O_2$ slides between the two straight lines B O_1, B O_2. In this case it is clear that any point O_3 attached to the bar O_1 P O_2, and distant a distance P O_3=P O_1 from P,

describes a straight line $O_3 B$ through B, the angle $O_3 B O_1$ being one half of the angle $O_3 P O_1$. This greatly adds to the usefulness of the linkworks.

Fig. 16.



It is not suggested that the linkworks here given exhaust 7-bar line linkworks; indeed it is obvious that many of them are susceptible of variations. Enough, however, has been shown to demonstrate the important part which the property of a quadrilateral combined with the principle of the conjugate image stated at the outset plays in the question.

I have strictly confined myself in this paper to the consideration of the rectilinear motion of a point. The principles involved, however, are applicable to many other problems, as, for example, the motion of bars every point in which moves in one of a series of parallel straight lines or in the same straight line. By means of the peculiar form of the fundamental linkage given in figs. 6, 13, 14, valuable results in the reversal or multiplication of angular motion may be obtained, and a linkage of $2n+2$ bars may be constructed which will divide any angle into n equal parts. Some of these questions have been treated of by me in my paper in the 'Messenger of Mathematics,' already referred to, which was, however, written before I obtained the general results here given. The extension of these results to other problems is reserved for future investigation.

XIX. "On the Heat of Sunshine at London during the twenty-four years 1855 to 1874, as registered by Campbell's Method." By H. E. Roscoe, F.R.S., and B. Stewart, F.R.S. Received June 10, 1875.

During the above period Mr. J. F. Campbell observed at London the relative heating effect of the sun in the following manner (described in the Report on the Warming and Ventilating of Dwellings, printed by order of the House of Commons, 25th August, 1857, p. 151).

A hemispherical cavity was made in a block of wood, and a spherical lens was made to be placed in this cavity in such a position that while its centre coincided with the centre of the cavity its chief focus was at some point of the hemispherical concave surface, the exact point being of course determined by the direction in which the rays struck the lens.

Whenever, therefore, the sun shone, a portion of the wood would be carbonized or burnt out by his concentrated beams; and inasmuch as the sun continually changes his position not only from hour to hour, but from day to day, it follows that different portions of the wood will be acted upon not only from one hour to another, but also from one day to another.

The blocks were all of mahogany, being as nearly as possible of the same quality, and the diameter of the sphere was about $5\frac{1}{4}$ inches.

When these blocks came into our hands, we considered how it was possible best to measure the amount of wood burnt out. At length we hit upon the plan of filling up the hollows burnt out with a mixture of bees' wax and olive-oil, of such a consistency that we could easily work it into the burnt cavities until the whole internal hemisphere should be made to present the same smooth surface which it had before it was burnt.

A comparison of the weight of the block before and after this process was supposed by us to afford an approximately good estimate of the extent of the hollows. Mr. J. A. Dodge, a student in Owens College, was good enough to assist us in this part of our research. Indeed he made two determinations for each block, dissolving out the mixture by means of heat and refilling the cavities; and the near concordance of these two determinations gives us reason to believe that the results are as accurate as the nature of the experiment requires. We subjoin a Table containing these results :—

TABLE I.

Individual Determinates of the Burnt-out Spaces.

Date.	First measurement.	Second measurement.	Mean.
Dec. 1854 to June 1855..	8·045	7·570	7·808
June 1855 to Dec. 1855..	3·580	3·320	3·450
Dec. 1855 to June 1856..	1·285	0·905	1·095
June 1856 to Dec. 1856..	1·330	1·115	1·223
Dec. 1856 to June 1857..	1·085	1·100	1·093
June 1857 to Dec. 1857..	0·360	0·529	0·445
Dec. 1857 to June 1858..	19·591	19·376	19·484
June 1858 to Dec. 1858..	23·210	22·697	22·954
Dec. 1858 to June 1859..	20·795	20·510	20·653
June 1859 to Dec. 1859..	31·296	30·440	30·868
Dec. 1859 to June 1860..	13·115	12·865	12·990
June 1860 to Dec. 1860..	19·340	19·525	19·433
Dec. 1860 to June 1861..	10·580	10·205	10·393
June 1861 to Dec. 1861..	16·920	17·030	16·975
Dec. 1861 to June 1862..	8·578	8·670	8·624
June 1862 to Dec. 1862..	20·600	21·170	20·885
Dec. 1862 to June 1863..	5·780	6·040	5·910
June 1863 to Dec. 1863..	24·310	24·100	24·205
Dec. 1863 to June 1864..	3·990	4·250	4·120
June 1864 to Dec. 1864..	15·290	14·985	15·138
Dec. 1864 to June 1865..	10·400	9·741	10·071
June 1865 to Dec. 1865..	18·180	17·900	18·040
Dec. 1865 to June 1866..	6·600	6·970	6·785
June 1866 to Dec. 1866..	19·225	18·785	19·005
Dec. 1866 to June 1867..	9·820	9·750	9·785
June 1867 to Dec. 1867..	23·425	23·495	23·460
Dec. 1867 to June 1868..	15·430	15·440	15·435
June 1868 to Dec. 1868..	13·970	13·540	13·755
Dec. 1868 to June 1869..	6·820	6·720	6·770
June 1869 to Dec. 1869..	24·105	24·780	24·443
Dec. 1869 to June 1870..	9·750	9·750	9·750
June 1870 to Dec. 1870..	22·170	21·830	22·000
Dec. 1870 to June 1871..	15·700	15·660	15·680
June 1871 to Dec. 1871..	16·880	16·690	16·785
Dec. 1871 to June 1872..	5·140	5·200	5·170
June 1872 to Dec. 1872..	14·550	14·460	14·505
Dec. 1872 to June 1873..	3·545	3·760	3·653
June 1873 to Dec. 1873..	25·470	25·605	25·538
Dec. 1873 to June 1874..	9·190	8·620	8·905
June 1874 to Dec. 1874..	20·745	20·400	20·573

Before proceeding further, it will be well to state that the first six results of the table were obtained by means of a water lens, while those that follow were obtained by a glass lens. The first six are not therefore comparable with those that follow.

Let us now endeavour to compare the heat of sunshine between the winter solstice and the ensuing summer solstice with that between the summer solstice and the ensuing winter solstice. The results of this comparison are given in the following Table :—

TABLE II.

Comparing the heat of sunshine during the six months preceding the summer solstice with its heat during the six months following the same.

Year.	Heat of the Sun during the six months	
	Preceding solstice.	Following solstice.
1855......	7·808	3·450
1856......	1·095	1·223
1857......	1·093	0·445
1858......	19·484	22·954
1859......	20·653	30·868
1860......	12·990	19·433
1861......	10·393	16·975
1862......	8·624	20·885
1863......	5·910	24·205
1864......	4·120	15·138
1865......	10·071	18·040
1866......	6·785	19·005
1867......	9·785	23·460
1868......	15·435	13·755
1869......	6·770	24·443
1870......	9·750	22·000
1871......	15·680	16·785
1872......	5·170	14·505
1873......	3·653	25·538
1874......	8·905	20·573
	184·174	353·680

From this table it appears, as might be expected, that the heat of sunshine is greater during the second than during the first half of the year, reckoning from the summer solstice; and this result is borne out by the observations made by one of us* that the chemically active solar rays are of greater intensity during the autumn than during the corresponding spring months.

There is a remarkable uniformity in this law, the only cases where it is broken being the years 1855, 1857, and 1868—years, it may be remarked, which are near the epochs of minimum sun-spot frequency. It thus appears that in order to compare one set of observations with another, it will be necessary to form yearly instead of half-yearly values; this is done in the following Table :—

* Phil. Trans. 1867, p. 562.

TABLE III.

Containing yearly values of the heat of Sunshine.

Mean date.	Amount.	Mean date.	Amount.
June 1855	11·258	June 1865	28·111
Dec. 1855	4·545	Dec. 1865	24·825
June 1856	2·318	June 1866	25·790
Dec. 1856	2·316	Dec. 1866	28·790
June 1857	1·538	June 1867	33·245
Dec. 1857	......	Dec. 1867	38·895
June 1858	42·438	June 1868	29·190
Dec. 1858	43·607	Dec. 1868	20·525
June 1859	51·521	June 1869	31·213
Dec. 1859	43·858	Dec. 1869	34·193
June 1860	32·423	June 1870	31·750
Dec. 1860	29·826	Dec. 1870	37·680
June 1861	27·368	June 1871	32·465
Dec. 1861	25·599	Dec. 1871	21·955
June 1862	29·509	June 1872	19·675
Dec. 1862	26·795	Dec. 1872	18·158
June 1863	30·115	June 1873	29·191
Dec. 1863	28·325	Dec. 1873	34·443
June 1864	19·258	June 1874	29·478
Dec. 1864	25·209		

The first five of these values must be treated by themselves, inasmuch as the lens was a water one. They point to a minimum of solar-heat action in 1856 and 1857; this agrees very well with the minimum of sun-spot action which took place in 1856.

Of the remaining thirty-three values the mean is 30·468.

Now there was a maximum of sun-spot frequency in 1859 and 1870, and a minimum about the end of 1866 or beginning of 1867.

Let us take the values of sun-heat action about these dates, and see if there be any correspondence. We have sun-heat action, mean date

Dec. 1858=43·607
June 1859=51·521
Dec. 1859=43·858
Mean of the above ....46·329,

a value which is greater than the average.

Again we have sun-heat action, mean date

Dec. 1869=34·193
June 1870=31·750
Dec. 1870=37·680
The mean of which is ......34·541,

which is also greater than the average, although the difference is not so decided.

Lastly we have sun-heat action, mean date

June 1866=25·790
Dec. 1866=28·790
June 1867=33·245
The mean of which is ......29·275,

which is less than the average.

It would thus appear that, as far as we can judge from these observations, there is more solar heat at London in years of maximum than in years of minimum disturbance.

This agrees very well with a remark made by Messrs. De la Rue, Stewart, and Lowey, the Kew Observers, to the effect that the number of fine days in the year on which solar pictures might be taken appeared to be greater in years of maximum than in years of minimum sun-spot frequency.

XX. "On the Effects of Iron Masts on Compasses placed near them." By Staff-Commander E. W. Creak, R.N. Communicated by Captain Evans, R.N., F.R.S., by permission of the Lords Commissioners of the Admiralty. Received June 17, 1875.

The question of the position of the standard compass on board ship, whether of wood or iron, is one of the greatest importance with respect to safe navigation. In H.M. ships it is one of the principal duties of the Superintendent of Compasses to secure the best possible position for this compass, and when that position has been determined, to ascertain the horizontal and vertical components of the total magnetic force resulting from the iron used in the construction and equipment of every class of vessel, in order that the correctness of this choice of position may be insured and the facts recorded.

With proper instruments in experienced hands these observations can be readily made; but in iron ships it is a somewhat intricate matter to eliminate the part which the hull plays in producing these forces, from those caused by iron used in equipment, unless observations are made on completion of the hull and afterwards as the equipment progresses. In wooden ships, however, the case is different; for the great mass of the ship being of wood and the iron in detached masses, the latter can be attacked in detail, and the disturbance they cause to the several compasses nearly observed.

Carefully conducted investigations made in different ships on the effects of similar bodies of iron are very valuable to those directing compass arrangements, as they give the necessary information as to how near those bodies may be approached or of necessity avoided.

The effects of introducing iron riders for strengthening wooden ships were in a measure discussed by the late Captain E. J. Johnson in his work 'On the Necessity of ascertaining the Deviation of the Compass,' where he treats of the diagonal iron riders of the 'Encounter' and 'Conflict,' but afterwards more fully in the papers which the present Hydrographer, Captain Evans, F.R.S., has written and published in the Philosophical Transactions of the Royal Society, Journal of the United Service Institution, and Transactions of the Institute of Naval Archi-

tects. In these papers may be traced in turn the effects of adding steam-engines and boilers, iron beams, and armour plating; but until lately no good opportunity has occurred for accurately defining the action of iron masts in producing compass disturbance from observation. The case referred to is that of H.M.S. 'Undaunted;' and when about to visit that ship in the ordinary course of my duties in the Compass Department, I was directed by the Hydrographer to make a special series of observations of the horizontal and vertical forces at all three compasses, as the lower masts and bowsprit were of iron.

To prepare the way for a discussion of the results of these observations, it appears necessary to give a short history of the two principal vessels mentioned in this paper, and also to go over some already well-trodden ground, whilst taking account of all the iron and the effects on their compasses, until the question of the iron masts alone remains to be settled.

For some years past there have been lying in Sheerness harbour two wooden frigates of exactly the same build, tonnage, and horse-power, namely, the 'Undaunted' and 'Newcastle'—the only important difference between them being that the 'Undaunted' has iron masts, the 'Newcastle' wooden. The diagonal iron riders which form the principal portion of the iron used in construction of their hulls are about 6 inches in breadth, $\frac{3}{4}$ of an inch thick, and placed 5 feet apart, at an angle of 45° with the decks. They extend from about 5 feet from the keelson on both sides of the ship up to the top sides—the after riders inclined towards the bow, the foremost towards the stern, the two sets meeting and overlapping amidships. These riders, therefore, being separated are independent magnets, except at the point of meeting just mentioned.

The 'Newcastle' was built at Deptford, head S. 73° W. (magnetic), and is a vessel of 3035 tons and 600 horse-power. The 'Undaunted' was built at Chatham, head S. 43° E. (magnetic), and is a vessel of 3039 tons and 600 horse-power. Assuming the dip at the time of building to be 68°, the after riders of both ships (near which the compasses are placed) were not far removed from the direction of the Earth's Total Force, and would therefore become strongly magnetized, especially those in the 'Undaunted.'

A glance at the coefficient C of the standard compasses given in the Table shows at once which ship was built in the easterly and which in the westerly direction; and, further, it is highly probable that in the 'Newcastle' the coefficient B would have been more nearly in accordance with the results of direction of the ship in building, but for the masses of iron introduced in equipment, such as engines and boilers, armament, great funnel, &c. In fact, as the compass is only 62 feet from the stern, there is a large excess of iron before that position compared with that abaft. We may reasonably assume the same of the B in the 'Undaunted' before her iron masts were stepped.

It is now time to say something about these iron masts. The bowsprit and foremast are so far removed from the compasses that their effect may be regarded as zero. The mainmast also being at a distance of 62 feet from the standard compass, its red and blue poles must neutralize one another. At the steering-compasses a slight repulsion towards the stern may exist, but sufficiently small to be neglected.

When, however, we consider the position of the mizzenmast, at a distance of only 8 feet 6 inches from all compasses, we shall find that it has produced deviations of very decided amount and marked character; and therefore our attention may be confined to that mast alone.

This mizzenmast, which is 82 feet in length and 24 inches in diameter, was built at Chatham of the "best-best" iron, half an inch thick, the plates overlapping making it 1 inch thick at its thickest parts, and for the purposes of this discussion may be considered a hollow cylinder. The effects of iron in that form have been already treated on mathematically by the late Mr. Archibald Smith, F.R.S., in the Phil. Trans. Roy. Soc. part i. 1865, pp. 317, 318.

Whilst building, the mast lay in a horizontal position, the direction N. 43° W. and S. 43° E., heel towards the northward, which was a favourable position for that end of the mast to receive a permanent quantity of red magnetism as the riveting was proceeding. When the mast was stepped in its nearly vertical position and subjected to the tremor caused by the rapid revolutions of the screw and engines, red magnetism would become more and more developed. To test to what extent this was accomplished the following experiments were made.

At 15 feet from the heel on the upper deck, and at 12 inches from the mast, a small compass was carried round, the blue pole of which pointed invariably to the mast. On the poop, at $22\frac{1}{2}$ feet from the heel and 12 inches from the mast, the blue pole of the compass was attracted three points. At $9\frac{1}{4}$ feet above the poop, and approaching the centre of the mast, there was no attraction; but the neutral zone was very limited in extent, as the blue magnetism began to make itself felt a few inches further up. This description of the mizzenmast shows that at 8 feet 6 inches from all compasses there exists a vertical, hollow, cylindrical magnet, the effects of which are clearly demonstrated by the following coefficients (p. 585).

According to the custom of Her Majesty's navy the ships were swung when ready for sea, to ascertain the deviations of the standard and steering-compasses. The vertical force, however, on board the 'Newcastle' was not observed, as in wooden ships experience has shown it to be so little disturbed. To remedy this deficiency I must now invite attention to another wooden frigate, H.M.S. 'Challenger.' When the 'Challenger' was about to start on her present voyage from Sheerness, an extended series of observations for the horizontal and vertical forces on board was made, in order to prepare for the correction of the magnetic observations now being carried out in her at sea.

Table of Coefficients computed from Deviation Tables observed at Sheerness.

	Name of Ship and Compass.	A.	B.	C.	D.	E.	Amount.	Force.	Starboard Angle.	λ.	μ.	χ.
		° ′	° ′	° ′	° ′	° ′	°		°			° ′
Standard.	Newcastle, 13. X. 74	−0 9	+ 7 6	− 2 16	+0 18	−0 2	7½	·128	342			
	Challenger, 6. XII. 72	−0 1	+ 6 28	+ 0 16	+0 23	−0 5	6¼	·112	2	1·003	·951	−0 7
	Undaunted, 29. III. 75	−0 14	+15 12	+ 2 53	+0 50	−0 2	15½	·269	10½	1·004	·831	−0 23
Starboard steering.	Newcastle, 13. X. 74	−0 57	+10 18	+ 0 23	+0 9	+0 59	10¼	·179	2			
	Challenger *, 6. XII. 72	+0 8	+ 6 7	+ 0 36	+0 16	−0 8	6½	·108	5	1·006	·840	−0 24
	Undaunted, 29. III. 75	−3 8	+26 58	+18 18	+0 42	+0 11	33	·555	34	1·005	·724	−0 39
Port steering.	Newcastle, 13. X. 74	−1 46	+10 25	−10 2	+0 32	+1 5	14½	·251	316			
	Challenger *, 6. XII. 72	+0 8	+ 6 7	+ 0 36	+0 16	−0 8	6½	·108	5	1·006	·840	−0 24
	Undaunted, 29. III. 75	−1 4	+25 52	−13 26	+0 52	−0 41	29	·495	332	1·010	·716	−0 41

* The position of this compass is in the centre line of the ship and the same height above deck as the steering-compasses; both of the latter may therefore be compared with it.

This vessel was built at Woolwich, head S. 16° W., and fitted out at Sheerness; and the coefficients obtained in her at two positions have been introduced to confirm and supplement those of the 'Newcastle,' especially as she is of the same construction, although smaller, and has wooden masts.

The notation and methods of computation of these coefficients are those given in the Admiralty Manual of the Deviations of the Compass.

The standard compasses are all within 3 inches of 12 feet 6 inches above the upper deck, the steering-compasses 3 feet 9 inches above that deck, and 6 feet apart.

An examination of the values given in the Table shows that about 8° have been added to the B of the 'Undaunted's' standard, but the C remains practically unaltered. This is evidently the effect of the red pole of the mast repelling the red pole of the compass towards the bow, and thus conspiring with the other iron of the ship to produce a large +B. Both the mast and compass being in the same fore-and-aft line, the C cannot be altered, and depends for its amount and sign on the diagonal riders before referred to. There is nothing unusual in the values of A, D, E, except that the 'Undaunted's' D ranks among the largest observed in wooden vessels.

The λ (or the ratio of the mean horizontal force on board to that on shore, Earth = 1·0) is at all the compasses a fraction above unity. This probably proceeds from the fact, already recorded, that the iron in these ships is in detached masses instead of its direction passing continuously through the compasses like iron beams, which invariably reduce λ below unity.

The μ (or the ratio of the mean vertical force on board to that on shore, Earth = 1·0) in the 'Challenger' shows how the red poles of the diagonal riders act in producing an upward force; but had the riders been joined at the keelson, it is almost certain that the μ would have been nearer unity at the expense of λ—a more desirable result, for this reason: experience has shown that λ may be reduced moderately below unity with no appreciable effect on the working of the compasses, whereas μ, upon which the heeling-error so mnch depends, should in wooden ships be as near unity as possible, heeling-error being troublesome to all navigators, and an insidious source of danger to the inexperienced.

In the 'Undaunted' the μ at all compasses is evidently much reduced by the vertical component of the force produced by the red pole of the iron mast, which pole is very strong at a distance of 16 feet, and at an angle of 35° from the vertical line passing through the standard compass.

The coefficient χ, or heeling-error, which in these ships is to leeward, and practically dependent alone upon the value of μ (λ being so near unity and D so small), of course becomes greater in the 'Undaunted' than in the two other ships.

So far all the coefficients at the standard compasses, and λ, μ, and χ at all compasses, have been examined; there remain therefore the A, B,

C, D, E of the steering-compasses to be considered. For this purpose there is sufficient evidence in the two sister ships, which are strictly comparable.

The compasses are necessarily placed out of the central line of the ships, and diagonally to the mast under discussion; therefore, as shown by the figures in the Table, a transverse component is introduced. In effect the iron mast has increased the B of the 'Undaunted's' steering-compasses by about 16°. To the +C of her starboard steering-compass about 11° have been added, and at the port steering a large −C of $13\frac{1}{2}$° shows the transverse component of the mast's force more strongly than at the starboard; but the $2\frac{1}{2}$° in excess are probably due to some other cause not accounted for by the observations made.

The A for these compasses is large in both ships; this does not, however, appear to proceed from magnetic causes, but rather from mechanical error in placing the binacles. D and E show the results usual in wooden ships, except the D in the 'Undaunted,' which is slightly increased.

Having thus, I think, defined the effects of the iron mizzenmast of the 'Undaunted' on her compasses, it remains now to show what was the most desirable way of meeting them, and what was actually done in that direction.

The most certain cure of the evil would have been to remove all the compasses further away from the mast, as far as magnetic reasons are concerned; but this would have entailed serious alterations in the arrangements of the ship which the occasion by no means warranted.

The standard compass might have been raised to a level with the neutral zone of the mast, but this at the expense of increasing vibration in the card from greater length of pedestal.

The steering-compasses being in the most suitable position for the use of the helmsman, and there being the standard compass as a means of comparison and obtaining the correct course, no change of position was necessary. The three compasses were therefore corrected by magnets, the semicircular deviation being reduced to the same amount as in the 'Newcastle.' It was not thought desirable to correct the whole of the semicircular deviation, as the mast, when the ship should make large changes of latitude, would probably add to the changing part of that deviation observed in this class of ships.

Conclusions.

The effects of iron masts are these:—

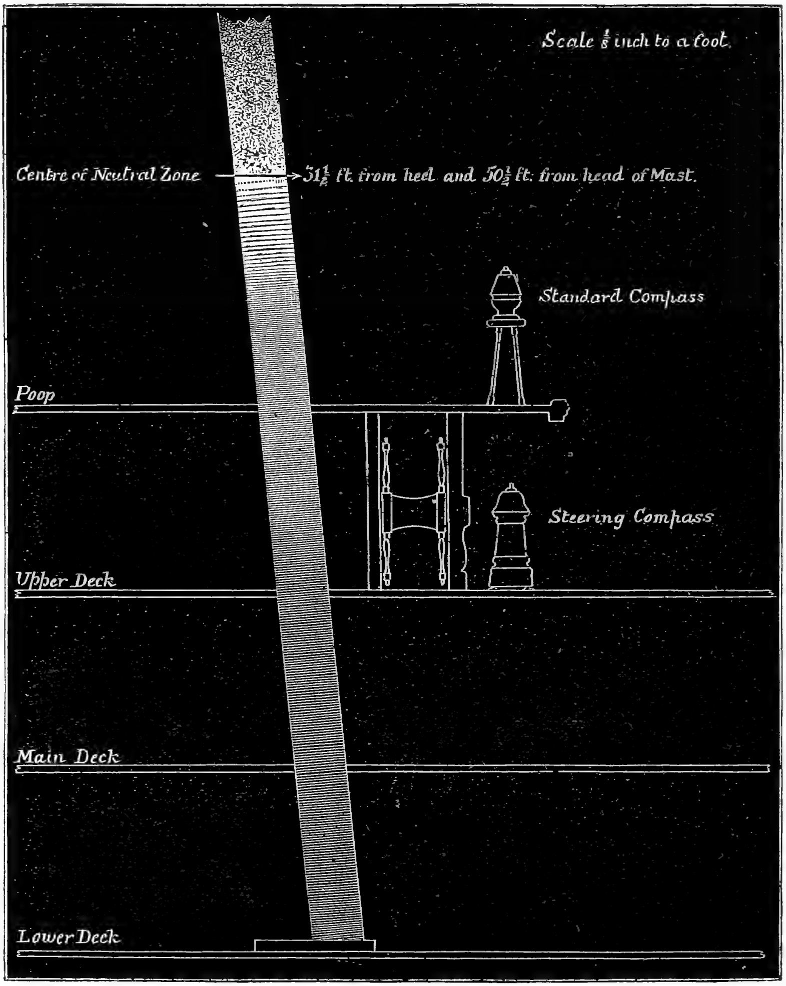
1. They produce semicircular deviation, and the objectionable addition to that deviation known as heeling-error.

2. That these effects need not always be avoided, as in certain cases they may be made useful in experienced hands; for example, the principal mast in this discussion (as shown in the accompanying sketch, p. 588) might, in an iron ship built head north, be used to correct the −B of

the standard compass, and oppose the downward pull of the ship's vertical force.

3. That the quadrantal deviation is slightly increased, and the mean directive force remains undisturbed.

4. Lastly, it is suggested that, as the magnetic condition of a mast may be easily ascertained by carrying a compass round it at stated distances and parts, the mast may be utilized or avoided as convenient. A similar examination of the mast in different latitudes would enable an observer to eliminate the effects of transient induced magnetism from the subpermanent. The amount of deviation proceeding from these causes is known for several classes of ships; but what part iron masts supply is yet a subject for inquiry.



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OBITUARY NOTICES OF FELLOWS DECEASED.

Hugo von Mohl, born April 8, 1805, was the fourth of five brothers, all of whom were men of note, either for public services or intellectual ability. His father was some time Minister at Wurtemberg for Home Affairs and Worship, while his mother, a person of exceptional gifts, was the daughter of Autenrieth, Finance Minister in the same State.

Von Mohl's early education was obtained at the Gymnasium of his native town, Stuttgart. In his nineteenth year (1823) he entered the University of Tübingen, where (in 1828) he graduated in medicine. In his inaugural dissertation (alluded to below) he clearly foreshadowed the course in science in which he was to preeminently excel. It was his father's wish that he should devote himself to surgery. This, however, was distasteful to him; and the intercourse into which he was thrown during the next few years with Von Martius, Zuccarini, Steinheil, and other botanists, soon determined the direction of his pursuits. In 1831 he contributed to the great work of Martius on Palms a memoir on the structure of the stems of those plants. In this year he was nominated first "adjunct" to the Botanic Garden of St. Petersburg, a post which, however, he did not accept, owing to his being appointed Professor of Physiology at Bern, whither he went in 1832. After the death of Schübler he returned, in 1835, to Tübingen as Professor of Botany in the University; and here he remained, notwithstanding many brilliant proposals tempting him elsewhere, till the time of his death. The interests of the University of Tübingen were matters about which he felt a keen solicitude, and the foundation of a Faculty of Natural Science in that University was essentially his work. In 1843 the Order of the Crown of Wurtemberg was conferred on him and he was ennobled. About this time he was obliged to make a prolonged stay in South Tyrol on account of delicate health. He recovered; but although a man of great stature and robust build, he appears, after he had accomplished his sixtieth year, to have fallen into chronic ill health. He suffered from pleurisy and attacks of diarrhœa. Eventually he became very reserved in manner and subject to giddiness. On the morning of Easter Monday, April 1, 1872, having been cheerful and well the night before, he was found dead in bed.

These particulars are derived from the memoir which appeared in the 'Botanische Zeitung' for 1872. Von Mohl was elected a Foreign Member of the Royal Society, March 26, 1868.

In describing fully Von Mohl's scientific career and position, it would be necessary to write the history of vegetable histology. His work is practically coincident with the application of the higher powers of the microscope to the investigation of vegetable tissue. Confining himself almost exclusively to the higher classes of plants, from the group of Muscineæ upwards (and neglecting the Algæ, Fungi, and Lichens), there is hardly a point of any consequence in which some research or investigation

of Von Mohl's is not the solid foundation of our present knowledge. The Catalogue of Scientific Papers of the Royal Society enumerates 78 of his papers—not including various dissertations, some of which, along with a selection of the more important of his papers, were in 1845 collected and published in a quarto volume, under the title of "Vermischte Schriften." The list of his publications which accompanies the memoir in the 'Botanische Zeitung' gives the titles of no less than 90. Nor were his own labours the only way in which he contributed to the advancement of our knowledge of the minute anatomy of plants. In 1843 he commenced, in conjunction with Schlechtendal, the 'Botanische Zeitung,' a small quarto weekly periodical of eight pages, occasionally illustrated with plates, which he continued to edit till the time of his death. The volumes of this journal chronicle, year by year, the gradual development of the microscopic study of plants, a field in which (doubtless in no small degree owing to the example of Von Mohl) German science has reaped a more abundant harvest than that of other nations. No one can fail to be struck with the thorough character of Von Mohl's scientific work. His energies were always ready to turn themselves to any part of his subject where facts seemed to need investigation, or the results of others to challenge reexamination or criticism. His papers are, in their way, models of "contributions to knowledge." Except when they are controversial, they always commence with a careful history and estimate of the work of previous investigators of the particular subject under consideration.

Von Mohl's first publication in 1827 was a prize thesis on the structure of climbing plants, in which he endeavoured to show that the stems have a dull kind of irritability, so that they bend towards any object which they touch. This explanation has given place to a better knowledge of the phenomena; but Mr. Darwin, to whom that service to science is largely due, bears witness to the *primâ facie* probability of Von Mohl's view (Journ. Linn. Soc., Bot. vol. ix. p. 10). His inaugural dissertation in 1828 (already alluded to) gave the first account of the true structure of the dots or "pores" frequently met with in the walls of cells ('Ueber die Poren des Pflanzenzellgewebes'). He showed that they were thinner portions of the cell-membrane.

In 1831 Von Mohl, as already mentioned, contributed to the 'Historia Naturalis Palmarum' of Von Martius an elaborate account in Latin of the structure of the stems and roots of palms, under the title "De Structura Palmarum." This was republished in German in his 'Vermischte Schriften' in 1845, and was translated for the Ray Society in 1849 by Prof. Henfrey. Von Mohl gave the final blow to the theory of the internal growth of monocotyledonous stems first propounded by Desfontaines, and upon which De Candolle had founded the division of vascular plants into Exogens and Endogens. In this memoir he appears to have first described the origin of ducts from rows of closed cells, a point which he further developed in the following year in a paper, "Ueber den Bau der porösen Gefässe.

In 1832 he also described the movements due to irritability in various species of *Robinia*. In a paper on Lenticels he disproved the theory of De Candolle that these structures were in any way dependent on the production of adventitious roots. His memoir on the stems of Cycads appeared also this year.

In 1833 he worked out the anomalous structure of the stomata in the Proteaceæ, which Robert Brown had regarded as imperforate and had described simply as "glandulæ cutaneæ." He contributed to the 'Icones Plantarum Cryptogamicarum Brasiliæ' of Von Martius a description of the anatomy of the stem of tree ferns. Hitherto the course of their vascular bundles had been supposed to be very similar to that of Monocotyledons. Von Mohl was the first to explain the structure of the hollow fenestrated cylinder into which the bundles are combined, and subsequent observers have added little to his account. In a memoir published in the 'Flora' he described the development of the spores in various of the higher Cryptogams. He gave the first accurate account of the development of the moss-capsule, pointed out the development of the spores in fours in one mother-cell, explained the development of the elaters of Jungermanniæ, and gave a correct account of those of *Equisetum*.

In 1834 he published an elaborate paper on pollen, in which he detailed an immense number of observations. It is true he fell into error in regarding the external coat as in some cases itself cellular. On the other hand, he was able to correct Robert Brown, who, in describing the peculiar movements (which have been called after him) in the granules of the fovilla of pollen, attributed to the granules at the same time a change of form. Von Mohl also established the development of the pollen-grains in fours within each mother-cell, thus indicating an analogy with the development of spores in Cryptogams.

The publication by Von Mohl in 1835 of his discovery of the multiplication of cells by division ('Ueber die Vermehrung der Pflanzenzellen durch Theilung') in *Cladophora glomerata* has been the starting-point of all subsequent investigations into the development of the tissues and organs of plants. It revealed, in fact, the precise mode by which vegetative *growth* is accomplished. Mirbel, in his memoir on the development of *Marchantia*, communicated to the Académie des Sciences in 1831 and 1832, but not published till 1836, had described the formation of pollen-grains by the quadripartite division of a mother-cell. This, however, though an extremely important observation, is not a case of growth, properly speaking, and does not affect Mohl's historical position in the matter. In 1838 Schleiden announced the multiplication of cells by the formation of new cells *in their interior* as a general law in the vegetable kingdom. He was supported by Nägeli. The views of Von Mohl, developed as they were by Meyen and Unger, eventually established themselves. In a paper on the structure of cork and bark, Von Mohl described the nature of the tissues which enter into their composition, and accounted for the

diversity of their character in different plants, especially the enfoliation of layers of bark in such trees as the Plane.

The following year (1836) Von Mohl developed his theory of an intercellular substance in which the cells of tissues were held to be imbedded. This supposed cement is now known to belong to the cell-walls themselves, and not to be a substance independent of them. Mohl indeed, later in life, practically withdrew his views upon the matter ('Die vegetabilische Zelle,' 1850). He also, in 1836, gave an account of the singular caudex of *Tamus* (*Testudinaria*) *elephantipes*.

In 1837 Von Mohl published the results of his investigations with regard to chlorophyl. They still remain essentially undisturbed. He showed that chlorophyl-granules were soft and homogeneous bodies, and not vesicular, as had been supposed; he detected in them the presence of starch. Nägeli having asserted that chlorophyl-granules possess a cellulose investment, Von Mohl returned to the subject in 1855, and showed that this view was untenable. During 1837 he also published a dissertation on the structure of vegetable membrane, in which he declared "secondary cell-membranes to possess a fibrous structure." The objective fact observed by Von Mohl is one of very general truth, although a rather different set of considerations are now brought to its explanation. His paper on the porous cells of *Sphagnum* finally put to rest the controversy as to their nature. He contributed an examination of some abnormal cones to the difficult question of the floral morphology of Coniferæ. In his observations on the sporangium in the vascular Cryptogams he established the real nature of this structure in ferns. In the case of *Selaginella*, Bischoff's view, that the sporangium is an axillary bud, was abandoned by Hofmeister for the explanation given by Von Mohl.

In 1838 he published the first account of the development of stomata, which he observed in *Hyacinthus orientalis*. He also corrected the account which Mirbel had given of the origin of the stomata of *Marchantia*. His dissertation, 'Ueber den Einfluss des Bodens auf die Vertheilung der Alpenpflanzen,' has served as a basis for the more extended investigations of Alphonse De Candolle on the same subject.

In 1839 he discussed the structure of annular vessels, contending against Schleiden that they were not a derivative form of spiral vessels. In his 'Die vegetabilische Zelle' he maintained a similar view with regard to reticulated and other vessels. He published some important observations on the development of the spores of *Anthoceros*.

In 1840 he described the peculiar phenomena of the growth of *Isoeteæ*; and, as Hofmeister has remarked ('On the Higher Cryptogams,' p. 337), "since Von Mohl's discoveries the special attention of botanists has been almost constantly directed to this interesting family."

In 1842 he published remarks on the structure of dotted ducts. In this he gave the first explanation of the structure of *bordered* pores. In the corrected form given to it by Schacht (1859) this is probably the

final account of the matter. In the same year he published his investigations on the cuticle of plants, which he demonstrated to be organically derived from the epidermic cells. He further supported this view in papers published in 1847 and 1849.

Von Mohl's researches in 1843 ('Ueber den Milchsaft und seine Bewegung') demolished Schultz's analogy of latex to blood. They were published in the 'Botanische Zeitung,' which was started in this year.

In 1844 Von Mohl maintained, against the theory of Dupetit-Thouars, the dependence of the growth of Dicotyledons on the physiological activity of leaves. The same year he published his remarks on the structure of the vegetable cell, which for a long time immensely influenced the course of vegetable histology. He regarded the cell-wall as generally composed of a primary external imperforate membrane, and a secondary one usually perforated with apertures. This he supposed to be lined by a third membrane, "Primordialschlauch," the primordial utricle of English writers. "This membrane forms a perfectly closed, cell-like, thin-walled vesicle, which in the fresh plant is closely applied to the inner wall of the cell, and therefore escapes observation; while in specimens which have been preserved in spirit it is contracted, and more or less detached from the wall of the cell."

In 1845 Von Mohl published a memoir on the Flora of Wurtemberg. It is interesting to find a great physiological botanist engaged in work of this kind, which it is rather the fashion at present to depreciate. Von Mohl enters at some length into the causes which influence the local distribution of plants; and it is in regard to points of this kind that he attaches scientific importance to local floras. An examination of a monstrous state of *Poa alpina* led him to the now generally received opinion that the lower floral glume of grasses is not a perigonial leaf but a bract. To the same year belongs a paper on the penetration of cuticle into stomata.

Von Mohl's paper, "Ueber die Saftbewegungen im Inneren der Zellen," published in 1846 (Bot. Zeit. p. 73), has been the starting-point of all modern views about the vegetable cell. He first described accurately the "opaque viscid fluid of a white colour, having granules intermingled with it, which fluid I call *protoplasm*." He observed the vacuolization of the protoplasm until it forms a mere network. He described the motion which takes place in the filament of the network, "or perhaps now first becomes visible," and he measured its rate. Schleiden gave the theory its finishing touch in the third edition of his 'Principles' (1849), by identifying Mohl's primordial utricle and circulating fluid.

In 1847 Von Mohl confirmed the researches of Amici (for whom he had a high regard) on the impregnation of Orchideæ, which, in his judgment, made an end of Schleiden's theory as to the origin of the embryo in Phanerogams, although the controversy was carried on for some time longer by Hofmeister, Tulasne, Schacht, and Radlkofer. He also published an elaborate memoir, which has been translated into both French

and English, in which he discusses against Mulder the value of the combined action of sulphuric acid and iodine as a colour-test for cellulose, his object being to insist on that substance being regarded as the basis of all vegetable membrane.

In 1850 he experimented with Hoffman on the function of vessels, but with contradictory results.

He pointed out in 1851 that the formation of chlorophyl is in intimate relation with protoplasm, portions of which it tinges in making its first appearance.

In 1852 he published a short paper on the grape-mildew.

In 1855 his researches on the structure of "liber" brought out some entirely new facts. He had already been the first (in 1836) to examine bast-cells with care. He pointed out that bast-cells were far from being so exclusive a constituent of the liber as had been supposed, and that they were often accompanied, or even entirely replaced, by the "Gitterzellen." His study of the causes of the opening and closing of stomata in 1856 was the first systematic examination which the question had received. It cleared up the apparent contradiction of the results obtained by Sir Joseph Banks on the one hand, and Moldenhawer on the other.

In 1857 Von Mohl discovered the extremely curious fact that the substance known as "gum tragacanth" is produced by the alteration of the cells of the pith and medullary rays of various species of *Astragalus.*

Passing over contributions of minor importance, Von Mohl published in 1870 a short discussion of the passages in the writings of Linnæus which have been held to indicate a foreshadowing in his mind of the theory of descent.

The last paper which appeared in Von Mohl's lifetime was on an extremely suggestive subject, the morphology of the foliar organs of the umbrella-pine of Japan (*Sciadopitys*). The microscopic structure of these organs led him to compare them to two consolidated leaves with the organic underside turned uppermost. They therefore suggested an analogy with the *squamæ fructiferæ* of Coniferæ generally, upon the true nature of which they have therefore been held to throw an important light.

In 1850 Von Mohl published a small work with the title "Die vegetabilische Zelle," which weaves the results of a great deal of what he had written in scattered memoirs into a continuous whole. It was translated into English by Prof. Henfrey in 1852.

Von Mohl felt the greatest interest in improving the means of histological and anatomical research, and wrote several papers on the construction and use of optical instruments, and in 1846 published a book on micrography.

Dr. Robert Edmond Grant was the seventh son of Alexander Grant, Esq., Writer to the Signet. He was born in his father's house in Argyle

Square, Edinburgh, on the 11th of November, 1793. His mother's maiden name was Jane Edmond. It appears, from a memorandum in Dr. Grant's handwriting, that he was sent from home to be nursed, and saw little of either of his parents during his infancy and childhood. He had eight brothers and three sisters, all of whom died before him; and as none of them left any children, Dr. Grant was the last survivor of his family.

When about ten years old he was placed at the High School of Edinburgh, where he continued for five years, under the tuition, successively, of Mr. Christison (afterwards Professor in the University), Dr. Carson, and Dr. Adam, the Rector, author of the well-known work on Roman Antiquities. In 1808 his father died; and in November of that year Dr. Grant became a student in the University of Edinburgh, attending the junior classes of Latin and Greek. In the following November he entered on his curriculum of medical study, and during its course attended the several classes in the Faculty of Medicine under the Professors of that day. He also studied Natural History under Professor Jameson, and attended the lectures of some of the extra-academical teachers. After completing his course of medical study, he in 1814 took his Degree of Doctor of Medicine, and published his inaugural dissertation, "De Sanguinis Circuitu."

In the mean time he had obtained (in May 1814) the Diploma of the College of Surgeons. In November of the same year he was elected one of the Presidents of the Medical Society of Edinburgh, a place justly regarded as an honourable object of ambition among the young aspirants in the Medical School.

Rather more than a year after taking his degree Dr. Grant went to the Continent, where he spent upwards of four years. During this time he visited various places of interest in France, Italy, and Germany, and made a pedestrian tour in Hungary; but his principal stay was in Paris, Rome, Leipsic, Dresden, Vienna, and Munich, on account, no doubt, of the varied opportunities for scientific study and general culture afforded by these foreign seats of science, art, and learning. He returned to Edinburgh in the summer of 1820, and took up his residence in his native city. At a later time he became a Fellow of the Edinburgh College of Physicians; but he seems not to have engaged in medical practice—his career had taken another direction. He had early imbibed a taste for Comparative Anatomy and Zoology, and now devoted himself assiduously to the prosecution of those branches of science, both by continued systematic study and by original research. The study of the invertebrate animals was peculiarly attractive; and at this time Dr. Grant published various interesting anatomical and physiological observations on mollusks and zoophytes; and his name will always be associated with the advances of our knowledge concerning the structure and economy of sponges, to the investigation of which Dr. Grant at this time enthusiastically applied himself. The pools left by the retiring tide on the shores of the Firth of

Forth afforded favourable opportunities for observation; and he would spend hours patiently watching the phenomena exhibited by these humble organisms in their native element.

Dr. Grant remained in Edinburgh till 1827, and in the mean time communicated the results of his various scientific inquiries to the 'Edinburgh Philosophical Journal' and the 'Memoirs of the Wernerian Society,' of which he became an active member. He was also (in 1824) elected a Fellow of the Royal Society of Edinburgh.

In June 1827 Dr. Grant was elected Professor of Comparative Anatomy and Zoology in the newly founded University of London, afterwards University College. He was not altogether new to the work of teaching. He had some early, though brief, experience in Edinburgh, in 1824, when Dr. Barclay, who for some years had delivered lectures on Comparative Anatomy during the Summer Session, entrusted him with the part of the course which related to the anatomy of invertebrated animals. He entered on his duties in London in 1828, and in October of that year delivered his inaugural lecture, which was published at the time and went through two editions. In this office he continued up to the time of his death, during which long period of forty-six academical years he never omitted a single lecture. This was a point on which he justly prided himself. Up to the last Session (1873–74) he continued to give five lectures a week; but, sensible of failing strength, he proposed to reduce the number to three in the next Session (which he was not destined to see). The number of pupils in his class fluctuated a good deal, but was never large, attendance not being compulsory in the Medical Curriculum prescribed by the Licensing Corporations. In one Session the number was fifty-six, but usually it was between thirty and forty, and sometimes much less.

After he had thus laboured for more than twenty years, the Council of the College added to the small return he received for his services an annual stipend of £100, which was continued during the rest of his incumbency. About the same time a number of his friends, in presenting him with a microscope in testimony of their esteem, purchased for him a Government Annuity of £50. Afterwards he succeeded to some property left by his brother Francis, an officer in the Madras Army, who died in 1852; so that in his latter years he found himself in easy circumstances.

His leading pupils were much attached to him, and he was sincerely esteemed and respected by all. His style of lecturing was clear and impressive, with a ready and copious flow of language. Without meaning to speak of his mode of treating his subject, we may nevertheless remark that on one great biological question (the origin of species) he was from the first an evolutionist, and on the promulgation of the Darwinian hypothesis of natural selection he became one of its warmest adherents.

In 1833 Dr. Grant delivered a gratuitous course of forty lectures on the structure and classification of animals to the members of the Zoolo-

gical Society. In 1837 he was appointed Fullerian Professor of Physiology in the Royal Institution, which office he held for the usual period of three years. At a later time he was appointed by the Trustees of the British Museum to the Swiney Lectureship on Geology, the tenure of which is limited to five years. In 1841 he delivered the Annual Oration before the British Medical Association. In 1836 he was elected a Fellow of the Royal Society of London. He was also a Fellow of the Linnean, Zoological, and Geological Societies.

Dr. Grant's vacations were spent sometimes in Scotland, but chiefly abroad, in France, Germany, Belgium, and Holland. On more than one of these occasions he was accompanied by an intelligent and favourite Hindoo pupil, Dr. Chuckerbutty, who afterwards became a Professor in the Government Medical College of Calcutta. Dr. Grant seems to have had a special liking to Holland, which he visited and revisited several times—partly, no doubt, on account of its scientific institutions and zoological museums, but largely also for the sake of acquiring the Dutch language. In like manner he afterwards spent his vacation in Copenhagen, and worked hard at Danish. Indeed it is to be noted that he had a great taste for the study of languages, both practical and philological, and spoke the principal European tongues fluently.

Dr. Grant's lectures were reported in the early Numbers of the 'Lancet' (1833–34), and he afterwards published a treatise on Comparative Anatomy which embodied the substance of them. The work came out in parts, but was not completed. He was also author of the article "Animal Kingdom" in Todd's 'Cyclopædia of Anatomy.' The titles and dates of his communications to periodical works are given in the Royal Society's 'Catalogue of Scientific Papers.' They are 35 in number, and extend from 1825 to 1839.

Dr. Grant was a devoted lover of music, and attendance at operas and concerts was one of his chief enjoyments in his latter years.

In August 1874 Dr. Grant suffered from a dysenteric attack, for which at first he would have no medical advice; and although subsequently, by appropriate treatment, the virulence of the disease was subdued, his strength was exhausted, and he died on the 23rd of that month at his house close by Euston Square. He was buried in Highgate Cemetery, attended to the grave by a few old friends and attached pupils, among whom was his friend and former companion in travel, Dr. Chuckerbutty, who was then in England, and two months later was destined to follow his venerated master.

Dr. Grant was never married. He knew of no surviving relatives. Three of his brothers, whose deaths he has recorded, were military officers. Of these, James, a lieutenant in the German Legion, fell at the siege of Badajoz in 1811; Alexander, captain in the Madras Engineers, died in the Burmese war in 1825; and Francis, captain in the Madras Army, as already mentioned, died at Edinburgh in 1852.

By his will Dr. Grant bequeathed the whole of his property, including

his collections and library, to University College, in the service of which he had spent the greater part of his life, and to the principles of which he was sincerely attached.

Sir John Rennie, C.E., past President of the Institution of Civil Engineers, was born August 30, 1794. He first assisted his father, the late John Rennie, in building both Southwark and Waterloo bridges. After the death of his father in 1821 he succeeded him as Engineer to the Admiralty, a post he held for ten years. Among his more important works are London Bridge (for which he received the honour of knighthood), Sheerness Dockyard, the completion of Ramsgate Harbour and Plymouth Breakwater (commenced by his father), the Earl of Lonsdale's Docks at Whitehaven, a portion of those at Cardiff, and the carrying out for a number of years of the great system of drainage and land reclamation in the Lincolnshire fens, also works at Newry and Dundalk. He was the author of a noble work on Harbours, of which Her Majesty was graciously pleased to accept the dedication, and for which he received tokens of honour from their Imperial Majesties the Emperors of Russia and Austria; also of a monograph on Plymouth Breakwater, and a small History of Engineering in the form of a Presidential Address to the Institution of Civil Engineers. In conjunction with his late brother, G. Rennie, he contributed to introduce the screw-propeller into the navy, and erected the machinery for the mints of Calcutta, Bombay, and Mexico; they also erected the Royal Clarence Victualling Yard at Plymouth; and Sir John Rennie was the first to perceive the uses of the diving-bell in engineering works. Sir John Rennie was admittedly the highest anthority on all subjects connected with hydraulic engineering, harbours, drainage, canals, irrigation, the storage of water, and the management of rivers; and his pamphlets on the drainage of Lombardy having attracted notice, the Italian Premier, Signor Sella, advised His Majesty the King of Italy to confer upon him the Order of St. Maurice and St. Lazare. Sir John long possessed a wide reputation on the Continent, as may be gathered not only from the above remarks, but from the fact that he constructed the harbour of Ponte Delgada in the Azores, that he was a Knight of the Tower and Sword of Portugal, of the Wasa of Sweden, and was also a Member of the Academy of Sciences of Stockholm and of the Austrian Society of Civil Engineers. Sir John was, further, well versed in general science and literature; and besides belonging to most of the scientific and learned societies in the metropolis, he was long an active Member of the Royal Society and the Meteorological Committee and one of the Council, and was also Chairman of the Juries at the Exhibition, 1862. Of late years, owing to age and increasing infirmities, he retired almost entirely from active life and public notice, and finally died, September 3, 1874. His kindness of heart won him many friends, who will greatly regret his loss.

Lambert Adolphe Jacques Quetelet was born at Ghent on the 22nd of February, 1796. He had the misfortune of losing his father at the early age of seven; and the poverty of his family obliged him to seek his own livelihood at once on leaving the Lycée. He obtained an appointment as teacher of mathematics, drawing, grammar, &c. in a school at Oudenarde. At the end of a year he returned to Ghent; and in February 1815, the very day on which he completed his nineteenth year, he was appointed to the Chair of Mathematics at the New College, which had replaced the Lycée. This appointment was not a brilliant one; but with the private lessons which he had the opportunity of giving, it afforded him a subsistence, and he had the satisfaction of feeling himself independent. He had even some leisure to devote to science, his flute, drawing, and to literary composition. About this time he, in conjunction with his intimate friend and former schoolfellow Dandelin, wrote an opera, entitled "Jean Second ou Charles Quint dans les murs de Gand," which was favourably spoken of. Dandelin soon afterwards left Ghent; and Garnier, who had become Professor of Mathematics at the University of Ghent, persuaded Quetelet to return to science. He studied the higher mathematics under Garnier, and at the same time assisted the latter by giving some of his lectures.

In 1819 he took the degree of Doctor of Science, the first conferred in that University. On this occasion he gave a brilliant inaugural address, in which he made known his discovery of a new curve of the third degree.

This discovery of the "focale" was much noticed in the 'Annales Belgiques' and in the 'Mercure Belge,' and was spoken of by Garnier and Raoul as a great honour to the newly founded university.

At the beginning of August, M. Falck, Minister of Public Instruction, came to Ghent, and was present at the laying of the first stone of the New University Buildings. Quetelet was on this occasion presented to M. Falck, and the strong recommendation of him by two such men as Garnier and Raoul led to his appointment shortly afterwards to a Professorship of Elementary Mathematics at the Athenæum of Brussels.

This appointment was made in the beginning of October; and by a private arrangement Quetelet engaged to give one quarter of his salary to his aged predecessor, M. Delhaye, as a retiring pension.

In Brussels Quetelet soon became intimate with the French refugees, David, Arnault, &c., besides frequenting the society of artists and literary men, and the theatres, where Talma, Mademoiselle Mars, &c. gave a series of performances each year.

At this period he composed various verses, and published, in the 'Annales Belgiques,' in the year 1825, an "Essai sur la Romance."

On the 24th of February Quetelet was made a Member of the Royal Belgian Academy of Science, receiving the diploma from Van Hulthem.

The first memoir he presented to the Academy, after his reception,

was on the 14th of October, 1820, and was entitled "Mémoire sur une Formule Générale pour déterminer la surface d'un polygone formé sur une sphère par des arcs de grands ou de petits cercles, disposés entre eux d'une manière quelconque."

The following are the titles of other papers, also read in the Academy, between December 1820 and February 1826:—

"Sur les Conchoïdes Circulaires." Note to "Mémoire sur les Caustiques." "Mémoire sur une nouvelle manière de considérer les Caustiques, produites soit par Réflexion, soit par Réfraction." "Résumé d'une nouvelle Théorie des Caustiques, suivi de différentes applications à la théorie des projections stéréographiques." "Démonstration et développements de la théorie des Caustiques secondaires."

Quetelet's researches, already noticed by Gergonne and other distinguished geometricians, were particularly remarked on by Chasles, after the 'Correspondance Mathématique et Physique' had given them greater publicity.

Two other memoirs by Quetelet, inserted in the collection of the Academy, still remain to be noticed—the "Mémoire sur quelques constructions graphiques des orbites planétaires" and the "Mémoire sur différents sujets de Géometrie à trois dimensions."

The 'Correspondance Mathématique et Physique,' already mentioned, was commenced by Garnier and Quetelet early in the year 1825. Its contributors were dispersed after 1830, and it came to an end in 1839.

In 1822 Quetelet went, at the request of the Academy, with M. Kickx to explore the celebrated grotto known as the Trou de Han, and they drew up a report with plates.

In 1824 it was proposed that Quetelet should extend his teaching at the Athenæum, so as to include Elementary Physics, Natural History, and Chemistry. About the same time M. Thiry, Professor of the Higher Mathematics, resigned his post; and during the session 1824 and 1825 we find Quetelet teaching at the Athenæum the Descriptive Geometry of Monge, the Theory of Shadows and Perspective, and the Calculus of Probabilities of Lacroix. He also gave public lectures at the Museum on Experimental Physics and on the Elements of Astronomy, which he had substituted for Natural History and Chemistry. He had two classes, which he conducted simultaneously in adjoining rooms, passing from one to another, and perfect order is said to have reigned in each. His teaching is described as simple and natural: his arithmetical instruction was founded on a few general principles; and as soon as his pupils were initiated into algebraic notation and its first rules, he showed them how this admirable instrument could be made to solve ordinary problems. His talent for drawing was evident in his manner of tracing his geometrical figures.

His courses of Physics and Astronomy at the Museum attracted large

audiences of all classes of society. He had a true gift for exposition, and could with very simple apparatus make himself clearly understood. In fact he objected to complicated instruments, and said of them that they often serve only to distract the attention from the results which it is the object of the lesson to explain.

Quetelet composed several elementary works for his public courses of instruction. The first in date, " On Elementary Astronomy," appeared in 1826 at Paris, in the 'Bibliothèque Industrielle de Malher,' and was frequently reprinted in France and Belgium, and translated into several languages. In 1827 he published a much more complete work, his 'Astronomie Populaire.' This latter was shortly followed by 'Les Positions de Physique,' which is considered superior to the 'Astronomie.' He endeavoured subsequently, in a little volume entitled "De la Chaleur," to put into practice his idea of founding instruction in Elementary Physics on experiments within reach of all. His intention was to follow this up by similar treatises on Magnetism, Electricity, and Light. Quetelet wrote the chapter on Acoustics in the 'Physique,' contributed by M. Plateau to the 'Encyclopédie Populaire.' Finally, he published in 1828 'Instructions Populaires sur le Calcul des Probabilités,' which was a *résumé* of the lessons he had been giving for several years at Brussels.

His public courses of lectures had been more and more successful ever since 1824. The government now deemed it advisable to organize other lectures of the same kind; and towards the end of 1826 the Administrator General Van Ewyck requested of Quetelet, on the king's behalf, a report on the matter, and on the 17th December the Museum of Science and Literature was established by a royal decree at Brussels. Quetelet was chosen for the Chair of Physics and Astronomy; but as he already gave courses of lectures on these subjects in his capacity of Professor at the Athenæum, he obtained leave to give a course on the History of Science at the Museum. He did not, however, long continue to give this course, as he left the Athenæum at the beginning of the year 1828, when he transferred his courses of Physics and Astronomy, which he had been giving at the latter institution, to the programme of the Museum, and these he continued to give until the close of the session 1833 and 1834.

The usefulness of the Museum was much diminished by the Revolution of 1830; and after languishing for a few years that institution was finally absorbed in 1834 by the Free University. A proposal was made to Quetelet to join the Free University; but this he declined, stating that he considered such an appointment to be inconsistent with his duties at the Observatory, to which he had been appointed on condition of not taking any other. He proposed, however, to continue his courses at the Observatory, those of Physics and Meteorology during the winter, and that of Astronomy during the summer evenings. This proposal was not acceded to; and Quetelet for a short time gave up public teaching, in

which he had already been engaged for twenty years. He, however, soon entered on it again, for, by a royal decree, Jan. 6, 1836, he was appointed Professor of Astronomy and Geodesy at the École Militaire. During this time he paid much attention to falling stars, and gave a method for determining the height of aërolites, by making simultaneous observations from different points. He also made experiments on the declination and inclination of the needle.

Quetelet had very earnest views on the subject of education, and he had twice occasion to make them publicly known—once under the Government of the Low Countries, once after the Revolution of 1830.

In the Commission instituted by King William in 1828, he was one of the small minority who wished to emancipate public instruction. He demanded a reduction of the number of Universities, and the establishment of two Polytechnic Schools—one for the northern provinces of the kingdom, the other for the southern provinces. Finally, he maintained that the time had come for substituting the use of modern languages for that of Latin, still in use.

Between 1823 and 1832 Quetelet was much occupied with Statistics; and the papers he published on this subject have perhaps contributed more than any others to popularize his name.

The first memoir was read to the Academy, and entitled "Mémoire sur la loi des naissances et de la mortalité à Bruxelles."

The second, also read to the Academy, "Recherches sur la population, les naissances, les décès, les prisons, les dépôts de mendicité, &c. dans le Royaume des Pays-Bas."

A third, also read to the Academy, "Recherches Statistiques sur le Royaume des Pays-Bas."

No new memoir appeared in 1830; but during the years 1831 and 1832 he devoted most of his time to Statistics, and published the following five memoirs on the subject:—

"Recherches sur la loi de croissance de l'homme." "Recherches sur le penchant au crime aux différents âges." "Recherches sur le poids de l'homme aux différents âges." "Recherches sur la reproduction et la mortalité," conjointly with M. Smits. "La Statistique des Tribunaux de la Belgique pendant les années 1826 à 1831."

In the memoir "Sur le penchant au crime," Quetelet worked out some of the ideas already made known by "Les Recherches Statistiques sur le Royaume des Pays-Bas."

He passed in review the different causes tending either to develop or to lesson the disposition to crime, and denied that instruction in reading and writing had the purely beneficial influence usually ascribed to it.

In enumerating his works we must not omit two other papers:—

"De l'influence des saisons sur les facultés de l'homme," and "Sur la possibilité de mesurer l'influence des causes qui modifient les Éléments Sociaux."

Quetelet had been deputed by Government to attend the Meeting of the British Association for the Advancement of Science which was to be held at Cambridge, beginning on the 25th of June, 1833. He went by Paris, where he read at the Institute his memoir on Mortality.

At Cambridge he took a warm interest in the establishment of the Statistical Section, of which Malthus, Babbage, and other *savants* became members.

In London he was summoned before an Inquiry Commission instituted by Parliament, to furnish information on the mode of keeping the Civil Registers of Belgium, and on the Census of the 1st of January, 1830.

Quetelet was one of the most active members of the Academy of Sciences at Brussels, and was always very desirous of promoting its independence. In 1834 he was appointed Permanent Secretary.

In 1835 he brought out an 'Annuaire de l'Académie.' About this time also he wrote for the British Association a paper of great interest, entitled "Aperçu de l'état actuel des Sciences Mathématiques chez les Belges."

Quetelet was appointed by the House of Representatives one of the Central Jury of Science. He retained these functions for some time, and showed great kindliness and sagacity in discharging them.

In the course of the year 1835 there appeared at Paris the chief of all Quetelet's works, "Sur l'homme et le développement de ses facultés, ou Essai de Physique Sociale." It was a *résumé* of all his previous works on Statistics.

In February 1836 Quetelet was charged with the execution of a Royal Decree for the establishment of a little meridian in the cities of Antwerp, Ostend, Bruges, Ghent, and Liége, and for placing a meridian-instrument on the walls of the Cathedrals, Hôtels de Ville, or other suitable buildings of forty-one different towns.

In August 1839 Quetelet made a journey, in company with his wife, in France, Italy, and Tyrol. His object was threefold. In the first place, he was to compare, in conjunction with his fellow commissioners, Messrs. Dumortier and Teichman, the standard weights and measures of Belgium with those of France; secondly, he was to attend the Congress of Savants at Pisa; and thirdly, he purposed to revise the determinations of magnetic intensity obtained in 1830, of the correctness of which he entertained some doubts.

At the sitting of the 7th December the Academy received a report of the proceedings of the Commission in the month of August; and Quetelet also presented the results of the Magnetic Observations which he had made in Tyrol and Italy.

In 1839 Quetelet communicated to the Academy a new Catalogue of the most remarkable appearances of falling stars—the second which he had made, for he had early turned his attention to this subject. He

appears, however, to have been in some uncertainty as to the nature of these phenomena.

The year 1839 was marked at the Observatory by the commencement of the observations on the flowering of plants, and in the month of January was begun the first of a series of monthly magnetic observations. These last observations were made at the suggestion of the Royal Society of London. It was in the year 1839 that Quetelet was elected a Foreign Member of the Royal Society; and in May 1841 these observations were considerably extended, and were thenceforth made regularly day and night at intervals of two hours.

The year 1841 was an important period of Quetelet's life. He thought that the time was past for individuals to promote the advancement of science by their isolated efforts, and that further investigations would need to be conducted by people associated together in academic bodies. In 1842 he drew up a set of instructions as to the choice of subjects for the reports. These instructions embraced Meteorology and Physical Geography and the Animal Kingdom.

In 1846 Quetelet published "Lettres à S.A.R. le duc regnant de Saxe Coburg sur la théorie des probabilités appliqués aux Sciences Morales et Politiques," which was reviewed by Sir John Herschel in the 'Edinburgh Review.' Soon afterwards he published a work entitled "Du Système Sociale et des lois qui le régissent."

The Revolution of 1848 turned Quetelet's thoughts to political questions; and in 1849 he read to the Academy a new note, entitled "Fragments sur la manière dont il convient d'envisager les Sciences Politiques et sur l'intervention du gouvernement dans les affaires des particuliers." He also presented to the literary section of the Academy a 'Note sur la Nature des États constitutionels, et sur quelques principes qui en dérivent.'

Quetelet had in 1841 organized a general system of observation of the periodical phenomena of vegetation. Five years later he attempted to solve the question as to the influence of temperatures on these phenomena; and he was led to appreciate the influence of heat, not according to the sum of the mean daily temperatures, as did Réaumur, but by the sum of their squares.

The results of these researches on the electricity of the atmosphere appeared in 1849. Two years afterwards Quetelet made an important investigation on the shape, size, and velocity of atmospheric waves, according to the observations made in June, July, and August 1841, and of the system of atmospheric waves of Central Europe, according to hourly observations of the summer solstice of 1841 and of the winter solstice of 1843.

This work on atmospheric waves was an important step in meteorology, and paved the way for the recent labours in international meteorology, to which we owe the remarkable law of tempests of M. Marié-Davy. The researches on the temperature of the earth, of which the

accounts were first inserted in the 'Mémoires' of the Academy, and those on the atmospheric waves, which had appeared successively in the 'Annales' of the Observatory, were afterwards united with other works on meteorology, and published under the title "Sur le Climat de la Belgique."

Quetelet married in 1825 a daughter of M. Curtet, a French physician, and niece of the well-known chemist Professor van Mons, a highly accomplished lady, by whom he had a son and a daughter. He was very hospitable, and entertained at his house persons of distinction who came to Brussels—artists, savants, literary men, and politicians of all parties.

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He continued to work during the remaining years of his life; and, in fact, when misfortune came heavily upon him by the loss of his wife, his daughter, and several of his grandchildren, work became his only consolation.

His son, M. Ernest Quetelet, had now taken the direction of the Observatory, and Quetelet occupied himself with Meteorology, Physical Geography, and Statistics. He continued to preside at the Central Commission, and assisted punctually at the International Congresses of Statistics, which were held in the great capitals of Europe. Six months before his death he made the fatiguing journey to St. Petersburg in consequence of a pressing invitation from the Grand Duke Constantine, under whose auspices the Statistical Congress was to be held. Neither the fear of cholera nor the anxious entreaties of his family could deter him from this enterprise. On his return he seemed refreshed, having been pleased by his reception. About this time also he had been made an Associate of the Academy of Moral and Political Science of the Institute of France; and ten days afterwards, in a congratulatory address sent to the Royal Academy of Belgium on the occasion of the hundredth anniversary of its foundation, the Academy of Sciences at Berlin proclaimed him the founder of a new science.

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Philippe-Edouard Poulletier de Verneuil was born at Paris on the 13th of February, 1805. He was educated for the magistracy, but the events of the year 1830 interfered with his plans. It so happened that just at this period of indecision about his future career geology was making a great advance. The fact was now becoming recognized that the earth's crust, far from having remained unchanged from remote antiquity, had undergone upheavals and fractures which had repeatedly caused alterations in the surface. In addition to this, even the relative ages of these different phenomena had been estimated. De Verneuil's interest was engaged by these results, and he studied under Élie de Beaumont the methods of investigation of geology; and resolved not merely to be a passive student of the works of others, but to devote himself to the extension of the science. With this view he made up his mind to travel, and selected Wales as the place of greatest interest since the publication of the researches of the two celebrated geologists Sedgwick and Murchison. These latter had recently established a certain order of superposition in the very thick group of the most ancient strata, which had hitherto been confounded under the general name of "Transition Rocks."

He next travelled in the East; and on the way to Turkey, on the Danube, fell in with some sympathetic companions, and with them he travelled through Moldavia and Bessarabia to Odessa, thence on to the Crimea and to the frontiers of Circassia, and later on to the Bosphorus.

He now published a memoir on the Crimea, which was supplemented and made more complete by a collection of fossils belonging to new and interesting species, which were described by M. Deshayes. Through the private instruction of this savant De Verneuil became intimately acquainted with the fossil shells, a subject of so much importance to stratigraphical geology.

In the year 1838 De Verneuil paid special attention to the lower strata of the Bas-Boulonnais district, and thereby acquired considerable authority in determining the fossils of the ancient rocks. It was on this account that when Messrs. Sedgwick and Murchison wished to compare the most ancient formations of the districts of the Rhine and Belgium with those of England, they invited De Verneuil to accompany them in their exploration. In the memoir they afterwards published they acknowledge the great assistance afforded by their companion in placing at their disposal his rich collections.

In conjunction with M. d'Archiac, De Verneuil published in 1841 a description of the fossils of the oldest deposits of the Rhenish Provinces. The work is preceded by a general survey of the fauna of the so-called Palæozoic rocks, and is followed by a table of all the organic remains hitherto met with in the Devonian System of Europe.

The results of this journey were so satisfactory that shortly afterwards Murchison again requested De Verneuil's company when he was plan-

ning the exploration of Russia. In the course of three summers, Messrs. Murchison, De Verneuil, and De Keyserling examined an area equal in extent to more than half Europe. They took different routes, meeting occasionally to compare notes.

The important work devoted to Russia in Europe and to the Oural Mountains, and accompanied by geological maps, appeared in 1845. The introduction of the Permian formation into the science was one of the chief results of this exploration. The whole of the second volume of this book is the work of De Verneuil, assisted by De Keyserling as far as the Palæozoic fauna is concerned. That part relating to the Secondary fauna was intrusted to D'Orbigny. Taking a general survey of the fauna of the four Palæozoic systems, the author shows that organized beings follow each other there in the same order as in the other countries of Europe.

De Verneuil next turned his attention to the New World. The North-American geologists had been working hitherto quite independently of those of Europe. They had ascertained the remarkable development of the ancient strata in that quarter of the globe, so remarkable for their immense thickness as well as for the considerable area they cover—no less than 35 degrees of longitude by 15 degrees of latitude. There were then no data for connecting the two systems, owing to the separation of the two continents by several thousand miles.

Scarcely was the publication of the results of the Russian expedition completed when De Verneuil undertook to supply this great want. The task before him was no less than to follow and compare on the two distinct continents the sedimentary deposits from the oldest fossiliferous strata to those containing coal.

As a foundation for this labour he had only those species which he had studied in local collections, or which he had himself found. He established the fact that even in such remote countries the first traces of life manifest themselves by nearly similar forms; and there is, indeed, a striking agreement in the order of their succession.

This notice on the parallelism of the Palæozoic strata of these two continents remains a standard work in spite of the constant advances of the science, and is perhaps the masterpiece of De Verneuil.

His next labours were in Spain. De Blainville, who did not believe in the universality of the laws of Palæontology, engaged him in this enterprise. Though the order of succession of the strata of Northern Europe and America were well established, this great naturalist supposed that in Spain, especially towards the South, the order of succession of the fossil strata must be reversed, or at any rate modified.

Between the years 1849 and 1862 De Verneuil made no less than twelve journeys in the Peninsula, sometimes alone, sometimes in company with M. Edouard Collomb, well known for his works on ancient glaciers, and sometimes accompanied by young naturalists desirous of

gaining instruction. In these various journeys De Verneuil collected a great number of fossils, and the laws of Palæontology already established received striking confirmation.

A geological map of Spain was published after these laborious excursions, besides some memoirs of great interest, especially one on the Primordial fauna.

De Verneuil had been elected in 1854 free Member of the Académie des Sciences. He was also a Member of the Royal Society and of other foreign Academies.

In his latter years he suffered from a weakness of sight.

He possessed singular clearness of judgment and freedom from prejudice, and could discuss with calmness and equanimity opinions directly opposed to his own.

In character he was extremely benevolent, and many acts of kindness are recorded of him. His modesty of disposition inclined him to prefer dwelling rather on the discoveries of others than on his own; and, in fact, it is not improbable that some of his fellow workers may from this very circumstance have failed to appreciate his full merit.

During his last illness, which was of three months' duration, he continued to take a lively interest in scientific and other questions. His cheerfulness never forsook him; and he died calmly on the 29th of May, 1873, at the age of 68. He bequeathed his unique collection of fossils to the Gallery of the École des Mines.

replaced by later issue

violently convulsed. The convulsions continued, almost without intermission, for three or four minutes, when death ensued. So far as could be observed, consciousness was not lost until immediately before death. The character of the convulsions resembled that of those produced by cinchonine or quinine, except that the tendency to backward movements, with the fore legs extended, was not so marked; they also resembled those produced by salts of the higher members of the chinoline series, but they were more severe than in the latter. The hydrochlorates of two condensed bases of this kind were employed—the first made from pyridine, and the other from picoline. The formulæ for these are:—hydrochlorate of dipyridine, $C_{10} H_{10} N_2$ 2HCl; and hydrochlorate of dipicoline, or parapicoline, $C_{12} H_{14} N_2$ 2HCl. The latter was found to be the more active of the two, but the actions were identical in character.

VI. General Conclusions.

1. There is a marked gradation in the extent of physiological action of the members of the pyridine series of bases, but it remains of the same kind. The lethal dose, however, becomes reduced as we rise from the lower to the higher.

2. The higher members of the pyridine series resemble, in physiological action, the lower members of the chinoline series, except (1) that the former are more liable to cause death by asphyxia, and (2) that the lethal dose of the pyridines is less than one half that of the chinolines.

3. In proceeding from the lower to the higher members of the chinoline series, the physiological action changes in character, inasmuch as the lower members appear to act chiefly on the sensory centres of the encephalon and the reflex centres of the spinal cord, destroying the power of voluntary or reflex movement; while the higher act less on these centres, and chiefly on the motor centres, first as irritants, causing violent convulsions, and afterwards producing complete paralysis. At the same time, while the reflex activity of the centres in the spinal cord appears to be so far inactive as not to be excited by pinching or pricking, it may be readily roused to action by strychnine.

4. On comparing the action of such bases as $C_9 H_7 N$ (chinoline) with $C_9 H_{13} N$ (parvoline), or $C_8 H_{11} N$ (collidine) with $C_8 H_{15} N$ (conia from hemlock), or $C_{10} H_{10} N_2$ (dipyridine) with $C_{10} H_{14} N_2$ (nicotine from tobacco), it is to be observed that, apart from differences in chemical structure, the physiological activity of the substance is greater in those bases containing the larger amount of hydrogen.

5. Those artificial bases which approximately approach the percentage composition of natural bases are much weaker physiologically, so far as can be estimated by amount of dose, than the natural bases; but the kind of action is the same in both cases.

6. When the bases of the pyridine series are doubled by condensation, producing dipyridine, parapicoline, &c., they not only become more

active physiologically, but the action differs in kind from that of the simple bases, and resembles the action of natural bases or alkaloids having an approximately similar chemical composition.

7. All the substances examined in this research are remarkable for not possessing any specific paralytic action on the heart likely to cause syncope; but they destroy life, in lethal doses, either by exhaustive convulsions or by gradual paralysis of the centres of respiration, thus causing asphyxia.

8. There is no immediate action on the sympathetic system of nerves, although there is probably a secondary action, because after large doses the vaso-motor centre, in common with other centres, becomes involved.

9. There is no appreciable difference between the physiological action of the bases obtained from cinchona and those derived from tar.

The physiological action of the substitution derivatives of these substances will be related in a further communication.

Transactions (*continued*).

Bombay:—Bombay Branch of the Royal Asiatic Society. Journal, 1873–74. No. 29. 8vo. 1874. The Society.

London:—British Pharmaceutical Conference. Transactions at the 11th Annual Meeting, held at London, August 1874; with Year-book of Pharmacy. 8vo. 1874. The Conference.

Entomological Society. Transactions for the year 1874. Part 4, 5. 8vo. The Society.

Oxford:—Ashmolean Society. Reports for the years 1872, 1873, 1874. In Memoriam John Phillips. 8vo. 1874. The Society.

Sydney:—Royal Society of New South Wales. Transactions for the year 1872. 8vo. 1873. The Society.

Vienna:—Kaiserliche Akademie der Wissenschaften. Denkschriften. Philosophisch-historische Classe. Band XXIII. 4to. *Wien* 1874. Sitzungsberichte. Phil.-hist. Classe. Band LXXVII. Heft 1–4; Band LXXVIII. Heft 1. Register zu den Bänden 1–70, zusammengestellt von Fr. S. Scharler. 8vo. 1874. Sitzungsberichte. Math.-naturw. Classe, 1874. Abth. 1. No. 4–7; Abth. 2. No. 4–7; Abth. 3. No. 1–7. Tabulæ Codicum Manu Scriptorum in Bibliotheca Palatina. Vol. VII. 8vo. *Vindobonæ* 1875. The Academy.

Clarke (Hyde) Researches in Prehistoric and Protohistoric Comparative Philology, Mythology, and Archæology. 8vo. *London* 1875. The Author.

Hart (H. Chichester) A List of Plants found in the Islands of Aran, Galway. 8vo. *Dublin* 1875. The Author.

Kirkman (T. P.), F.R.S. Philosophy without Assumptions. Part 3. 8vo. *Liverpool* 1873. The Author.

Prestwich (J.), F.R.S. The Past and Future of Geology: an Inaugural Lecture. 8vo. *London* 1875. The Author.

Schoof (W. G.) Improvement in the Lever Escapement for Watches and Marine Chronometers. 8vo. *London* 1874. The Author.

Serret (J. A.) Traité de Trigonométrie. 5e édition. 8vo. *Paris* 1875. Traité d'Arithmétique. 6e édition. 8vo. *Paris* 1875. The Author.

Vincent (C. W.) The Year-Book of Facts in Science and Art for 1874. 8vo. *London* 1875. The Publishers.

The mean of the coefficients of linear dilatation of silver between 0° and 100° C., given by various authorities, is

$$0{\cdot}00002015.$$

It will thus be seen that the expansion of silver between 0° C. and 1050° C. is about twice as much as it would have been had this rate of expansion been maintained through the whole range of temperature.

The mean coefficient of linear dilatation of Levol's alloy, as deduced from the results given in the Table, is

$$0{\cdot}00003703\,;$$

but it is impossible to compare this with the rate of expansion at low temperatures, as the latter has not been ascertained.

	Initial volume of cone.	Volume of cone filled with molten metal.	Temperature of metal when poured.	Weight of metal.	Density when fluid.	Density of solid metal.
	c. c.	c. c.	°C.	grms.		
Pure silver.	536·6	556·3	1143	5255·4	9·4468	10·57
	542·9	564·4	1223	5348·3	9·4757	
	Mean				9·4612	
Levol's alloy.	735·13	778·06	1020	7062·4	9·0788	9·9045 [Levol], 9·998 by calculation.
	537·42	557·25	1131	5033·4	9·0321	
	Mean				9·0554	

In conclusion I have much pleasure in acknowledging the assistance I have received from one of the Assistant Assayers, Mr. Edward Rigg, whose cooperation has been of much service to me; and I must also express my thanks to Joseph Groves, Senior Fireman, who aided me in the furnace-operations.

Correction to Prof. CAYLEY'S "Eighth Memoir on Quantics." Phil. Trans. Vol. 157 (1867). Received June 26, 1875.

The Table for L, M, L′, M′, p. 544, should stand :—

$72\,\mathrm{L}=$		$72\,\mathrm{L}'=$	$24\,\mathrm{M}'=$
⋮		$\mathrm{A^3I}$ +1	I −1
		ABI +3	
$\mathrm{A^2B^2C}$ −81		CI −15	

and substituting these values we find for 36a, 36b, &c. the values given p. 554; where in the expression of 36a, the term $\mathrm{A^2B^2C} - 126$ should have been distinguished by an asterisk, to show that there was an alteration in the coefficient, −126, instead of −36 as given p. 544.

accounts were first inserted in the 'Mémoires' of the Academy, and those on the atmospheric waves, which had appeared successively in the 'Annales' of the Observatory, were afterwards united with other works on meteorology, and published under the title "Sur le Climat de la Belgique."

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PROCEEDINGS OF

THE ROYAL SOCIETY.

VOL. XXIII. No. 156.

CONTENTS.

[*For continuation of Contents see 4th page of Wrapper.*]

November 30, 1874.

ANNIVERSARY MEETING.

NOTICE.

A Meeting of the Government-Grant Committee will be held in February. It is requested that applications to be considered at that Meeting be forwarded to the Secretary of the Royal Society before the 31st of January, 1875.

TAYLOR AND FRANCIS, RED LION COURT, FLEET STREET.

PROCEEDINGS OF

THE ROYAL SOCIETY.

VOL. XXIII. No. 157.

CONTENTS.

December 10, 1874.

December 17, 1874.

[*For continuation of Contents see 4th page of Wrapper.*]

ERRATA IN VOL. XXII.

Page 242, line 8 from bottom, *for* Co Cl_2, $2H_2O$, *read* Co I_2, $2H_2O$.
" " " 7 " *for* Co Cl_2, $6H_2O$, *read* Co I_2, $6H_2O$.

NOTICE.

A Meeting of the Government-Grant Committee will be held in February. It is requested that applications to be considered at that Meeting be forwarded to the Secretary of the Royal Society before the 31st of January, 1875.

PHILOSOPHICAL TRANSACTIONS.

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NOTICE.

A Meeting of the Government-Grant Committee will be held in June. It is requested that applications to be considered at that Meeting be forwarded to the Secretary of the Royal Society before the 31st of May, 1875.

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June 10, 1875.

ERRATA.

Page 398, line 18, *for* ·21560 *read* ·21506.

„ head of column 3 of Table, *for* $12\frac{}{n}$ *read* $12\frac{r}{n}$.

Page 405, line 25, *for* $//c=b=\backslash\backslash c\sharp$. *read* $//c=6$[six]$=\backslash\backslash c\sharp$.

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